1976

The Secondary Qualities In A Physicalist System Of Philosophy

Ransom Everett Slack

THE SECONDARY QUALITIES IN A PHYSICALIST SYSTEM OF PHILOSOPHY

by

Ransom Everett Slack

Department of Philosophy

Submitted in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy

Faculty of Graduate Studies
The University of Western Ontario
London, Ontario
July, 1975

ABSTRACT

This thesis attempts to fulfill a dual purpose, and hence can be read from either of two points of view. On the one hand, it is an attempt to develop some key aspects of a physicalist system of philosophy. On the other hand, it seeks to solve the problem of the secondary qualities. This latter problem is that presented by the difficulty of clarifying the ontological status of the secondary qualities in the context of physicalism. This dual role is rendered coherent by the fact that each aspect depends on the other: A major obstacle to the development of an adequate physicalist system of philosophy is the problem of secondary qualities; and the solution of that problem requires the application of various metaphysical, epistemological, and semantic principles.

Some key features of the philosophic system developed here are these: The metaphysical position of realism is defended, both with regard to the reality of that which is not experienced, and with regard to the reality of universals (kinds). The basic features of this reality are taken to be properties and their tokens. Properties are classified as being either relational or intrinsic, and the latter are further classified as either structural or non-structural. Ultimately, the traditional distinction between primary and secondary properties is repudiated. In the domain of epistemology, the existence of percepts is defended against various criticisms, particularly those of D. M. Armstrong. However, the type of representationism defended differs from traditional

views of this sort in that it treats the percept as a theoretically posited physical entity in the brain. Also, it is argued that our direct acquaintance with these perceptual contents cannot serve as a foundation for an edifice of knowledge, despite the fact that there are respects in which we cannot be mistaken concerning the character of these perceptual contents. In developing these views, a general theory of error, a theory of explanation, and a theory of induction are presented. In the domain of semantics, a theory of meaning as use is sketched, and its implications for the analytic-synthetic distinction are briefly explored. Also, it is argued that the use of subjunctive conditionals carries with it ontological committments.

The problem of secondary qualities is dealt with in some detail. To start with, its cognizance by other theorists working in the field is displayed, and then two well-developed attempts to solve it are criticized: these are the attempts of D. M. Armstrong and C. A. Hooker. Finally, a solution for the problem is proposed which draws on the metaphysical classification of properties, the theory of percepts, and the theory of meaning developed earlier in the thesis. This discussion includes a careful look at the criteria applicable to successful contingent identifications. The specific character of the proposed solution to the problem of secondary qualities is this: The secondary qualities, since they are non-structural intrinsic properties, can be contingently identified with non-structural intrinsic properties in the brain,

particularly since our physical theories tell us nothing concerning the intrinsic character of the latter. To help add substance to this proposed solution, a bit of speculation is done concerning a possible physical correlate for colour.

ACKNOWLEDGEMENTS

I am especially grateful to two people for their help with this endeavor. To Cliff Hooker, teacher and friend, I owe years of inspiration in coursework and discussions, and also much very valuable criticism of the present work in its earlier forms. I hope that I have not been unfair in the criticisms of his views which I present in this thesis, and there is no doubt that he can not be held responsible for the errors which are bound to remain in my own work.

To Gisela Piege, secretary and friend, I owe the many hours of work she has devoted to typing the final draft of this thesis, and I am grateful for her patience with the retyping she had to do because of my own errors.

TABLE OF CONTENTS

	Page
CERTIFICATE OF EXAMINATION	ii
ABSTRACT	iii
ACKNOWLEDGEMENTS	vi
TABLE OF CONTENTS	vii
LIST OF FIGURES	x
Chapter	
1. INTRODUCTION	1
2. GENERAL PRESUPPOSITIONS	6
DEFENSE OF REALISM	7
NO FOUNDATIONS	20
REALITY OF KINDS	28
Nominalism	29
Conceptualism	34
Positive Account	38
3. METAPHYSICS	40
INTRINSIC AND RELATIONAL PROPERTIES	40
STRUCTURAL METAPHYSICS	48
Bare Particulars Metaphysics	49
Infinite Hierarchy of Structures Metaphysics	51
Form-and-Content Metaphysics	52
STRUCTURAL METAPHYSICS IN MATHEMATICS AND PHYSICAL SCIENCE	54
Mathematics	54
Physics	57

Chapter Page

4. THE NEED FOR PERCEPTS 72

POSITIVE ARGUMENT 73

REPLIES TO OBJECTIONS 76

Miscellaneous Objections 76

Armstrong's Objections 87

5. ERROR AND CERTAINTY IN PERCEPTION 96

THEORY OF ERROR 97

SYNTHESIS 106

NON-FOUNDATIONAL EXPLANATION AND INDUCTION . 109

Explanation 109

Induction 115

The Justification of Induction 131

6. DISPOSITIONS AND MEANINGS 140

DISPOSITIONAL AND MANIFEST PROPERTIES . . . 140

ASPECTS OF A THEORY OF MEANING 147

The Analytic and the Synthetic 147

The Uses of Terms and Ontological Commitments 153

7. THE PROBLEM OF SECONDARY QUALITIES 159

STATEMENTS OF THE PROBLEM 159

ARMSTRONG'S ATTEMPTED SOLUTION 167

HOOKER'S ATTEMPTED SOLUTION 186

8. A PROPOSED SOLUTION FOR THE PROBLEM 202

CONTINGENT IDENTIFICATION 205

Semantic Issues 205

Chapter Page

Ontological Issues 210

A SPECULATIVE CORRELATE FOR COLOUR 225

ABOLITION OF THE PRIMARY-SECONDARY QUALITY DISTINCTION 230

APPENDIX 232

NOTES 242

BIBLIOGRAPHY 252

VITA 255

LIST OF FIGURES

Figure	Description	Page
1	An Illustrative Structure	42
2	The Context of Error	99
3	The Visual Pathway	217
4	A Primitive Mechanism with Expectations	232

Chapter 1

INTRODUCTION

This thesis aims to fulfill a dual purpose, and hence can be read from either of two points of view. On the one hand, it is an attempt to construct some key aspects of a systematic physicalist philosophy. On the other hand, it is an attempt to solve a single key problem for the development of physicalism: the problem of secondary qualities.

The problem of secondary qualities is this: How can the secondary qualities, which are seemingly undeniable aspects of our experience, be fit into a coherent physicalist account of reality? Ever since Galileo, the accounts of physical science have been restricted to appeals to primary qualities, and the secondary qualities have had no role to play in the laws of physical science. Thus the secondary qualities have been traditionally relegated to the mental realm of a dualist metaphysics. But once physicalism is adopted, and in particular, once the mind-brain identity theory is adopted, the physical laws, with their exclusion of the secondary qualities, invade the realm of the mind. The ontological status of the secondary qualities then becomes problematic.

How then can this thesis fulfill a dual role and yet pretend to maintain a semblance of unity? How this can be done can be indicated by looking at each aspect of this dual

role in turn. If, on the one hand, we regard the thesis as primarily an attempt to construct some key aspects of a systematic physicalist philosophy, we can hardly avoid the problem of secondary qualities. If such a system is to have any hope of success, it must show the way to the solution (or dissolution) of that problem. If, on the other hand, we regard the thesis as primarily an attempt to solve that problem, we find that in order to provide a satisfactory solution to the problem we must consider wide-ranging issues in metaphysics, epistemology, and semantics. For instance, we will find a need to delve into such issues as the role of form-content distinction in metaphysics, the existence of percepts in epistemology, and the role of meanings in contingent identifications. Thus, in approaching the dual roles of this thesis from either direction, one is led into the need for the other role. The ideal will be to maintain this standard of coherence throughout.

In an attempt to achieve such coherence, the thesis will be developed along this sequence: It will begin (Chapter 2) by laying some very general metaphysical and epistemological groundwork. This will then serve as a context for later developments. This groundwork will consist in arguments for metaphysical realism and for a non-foundational epistemology. With this groundwork laid, the thesis will proceed (Chapter 3) to set up some metaphysical principles which will be needed in the solution to the problem of secondary qualities. These principles will be those deline-

ating a form-and-content type of metaphysics. To provide specificity for this type of metaphysics, its consequences for the interpretation of mathematics and physical science will also be explored. This will serve to clarify how the metaphysical principles of the thesis are to be applied, and will also display more key points which will be necessary for the solution to the problem of secondary qualities.

In Chapter 4, we will depart from metaphysics, and move into a discussion of some principles of epistemology. In particular, an effort will be made to demonstrate the theoretical need for percepts, and for the correlative concept of acquaintance. These concepts will also be of the utmost importance in developing my proposed solution to the problem of secondary qualities. Chapter 5 offers some further refinements of the epistemological principles developed in the previous chapters, and many of these refinements are required to demonstrate the consistency of those principles. To help round out the epistemology of this system, theories of explanation and induction are also presented here.

In Chapter 6, basic semantic principles are sketched for the sake of providing further backing for claims made earlier in the thesis, and for the sake of providing a tool with which to handle the semantic issues which arise in the contingent identifications required by my proposed solution to the problem of secondary qualities. That problem is then given a clearer statement in Chapter 7. Here also a couple of attempts to solve the problem are examined, and found

wanting. Finally, in Chapter 8 I present my proposal for solving the problem of secondary qualities, drawing on the metaphysical, epistemological, and semantic principles which had been argued for in the previous chapters of the thesis.

As a development of a physicalist philosophy, this work must, of necessity, deal with some issues in a rather sketchy manner. Considerable effort has been taken to defend those views which seemed in most need of defending, although I recognize the inevitability of differences of opinion as to what requires extensive defense and what does not. However, in defense of the presence of some sketchy work in this thesis, I wish to point out that such sketches serve as good points of departure for further research leading to further development of a more complete physicalist philosophy.

Again, due to the need to restrict the length of a work such as this, and also for the sake of concessions to coherence, very important aspects of a complete physicalist philosophy have been left untouched here. A particularly important aspect of any philosophic system is value theory, which is not even mentioned anywhere in this thesis (except, of course, right here). As might be expected, I would hope to develop a general theory of value along naturalistic lines. My point of departure for such a theory will be, I expect, the points concerning purposes touched on in the Appendix. In any case, I believe that the development of a general theory of value is incidental to the main organiza-

tional principle of this thesis, which is the development of the principles of a physicalist philosophy which are more or less requisite in the adequate defense of my proposed solution to the problem of secondary qualities.

Chapter 2.

GENERAL PRESUPPOSITIONS

Any philosophic system is developed in the context of certain sorts of answers to very general philosophic issues. Answers to some of these issues will be presupposed in the main arguments of this thesis. In this chapter, I wish to offer some arguments in support of these presuppositions. The issues with which I shall concern myself here are: (1) realism versus solipsism and phenomenalism, (2) foundational versus non-foundational epistemologies, and (3) the problem of universals.

The main reason for setting the discussion of these issues apart from the rest of the thesis is that the structure of the arguments used in this chapter are, in general, of a different sort from those developed in the other chapters. Most of the arguments of the later chapters are developed within the context of the system, and hence can appeal to previously-established principles of the system. But the arguments of this chapter are not of that sort. Since the answers we give to these issues are presupposed in the further development of the system, there is not really much within the system that can be appealed to without begging the question. But simply stating a position on a controversial issue is not very satisfactory either. One likes to think that one's position is more reasonable than

some other positions. So we seek arguments to display the advantages of our own system. The type of arguments that are available for such an enterprise are arguments which are directed at specific sorts of opponents. We seek the beliefs held in common between our opponents and ourselves, and attempt to use those beliefs as leverage in displaying the advantages of our preferred system. I shall be following such methods in this chapter.

Of course, there are no guarantees in using such a method, either with respect to certainty, or with respect to convincing opponents. On the one hand, principles agreed to may be false; and on the other hand, the opponents may decide to abandon the agreed-to premises rather than submit to the conclusion. Despite these drawbacks, I do feel the effort is worth making.

The division between types of arguments which I drew above is not a sharply defined one. It will often be the case that arguments of this "external" nature may occur in the context of more advanced stages of the development of a philosophic system. Indeed, most attempts at empirical confirmation would best be regarded as such arguments. However, the distinction is sharp enough to merit our attention, and is helpful in clarifying how one can get a philosophic system off the ground in a reasonable manner.

DEFENSE OF REALISM

The system which I will partially develop in this

thesis is a realist system of philosophy. Now there are many senses in which the term "realism" has been used in philosophy. So first of all, we can note that there is a trivial sense of realism, with regard to which all philosophical systems must be classified as realist. I will refer to this as trivial realism. It involves commitment to this claim:

(P1) There is an entity (or entities) which exists, or which has (have) being, or which is (are) real.

Even solipsism qualifies as realist in this sense, since it asserts the reality of personal experiences.

Because this sense of realism is so pervasive, we need bother with it little. But it does allow us to focus on the sort of issue with which we are concerned when we do metaphysics. The issue is this:

(Q1) Granted that some entity (or entities) exists, which entities exist, and how are they to be characterized?

This is the fundamental issue of metaphysics. Correspondingly, there is a fundamental issue of epistemology:

(Q2) How are we to decide which entities are real, and what their characteristics are?

Thus we see in trivial realism a basis of agreement, from which disputes may start. We all agree that something exists. Now the task is to determine the character of reality: Which entities (proposed) exist, and which do not?

Are there minds, ideas, universals, matter, classes, gods, electrons, persons, justice, values, tables, etc. etc.? The task of metaphysics is, then, to determine, by the examination of evidence and arguments, the character of reality. Epistemology may, in general, then be regarded as having the task of determining what is to count as evidence, and what constitutes proper argument.

The system of philosophy being developed here is realist in more than the trivial sense. It is realist as opposed to phenomenalist. That is, it asserts:

(P2) There is an entity (or entities) which exists, has (have) being, or is (are) real, entirely independently of being experienced.

Phenomenalism asserts the contradictory of this, and hence may be characterized as asserting "esse est percipi" or, "to be is to be perceived". Phenomenalism, as understood here, defines existence in terms of experience. Realism offers no such definition. Indeed, it does not define "exists" or "is real" at all. These are primitive terms of the system. If they are not understood, they cannot be explained within this system.

Given that (P2) is a claim of some controversy in philosophy, I feel compelled to argue for it. Now let me remind the reader of the character of the argument I am going to give. It is not to be an argument of the type Descartes sought. That is, it is not an attempt to establish (P2) on the basis of *a priori* indubitable premises. As we shall see

later, the system being developed here is anti-foundationalist, and hence does not allow for such arguments. But rather than merely asserting the key claims of the system, I feel some obligation to argue for them. The form of the arguments that are possible and legitimate in such a context is this: Premises and principles of inference will be only those to which opponents might agree. And because it is phenomenalists I must argue with here, we can only hope to find common ground for argument in appeals to the character of our experiences. We share this common ground, since my realism involves the belief that experiences are part of reality.

The characteristic of our experience which interests us here is this: Much of our experience has a character which is independent of our control, expectations, or desires. For example, we can will to open our eyes (if you prefer: we can cut off or produce visual experiences at will), but we find ourselves unable to control our experience once we do so. We can will to look in certain directions, but once we do so, we have little or no control over what we see. We cannot will what we hear, once our ears are not plugged; we cannot choose what we feel, once we place our hands in a given position; and so on, for each of the senses. I am not disputing here that there are aspects of our experience which we do control (on-off, selection among available stimuli, etc.). But what is of importance here is that there are aspects of our experience which we do <u>not</u> control.

This characteristic of experience is not limited to the experiences of waking life. Most hallucinations and dreams display the same characteristics of independence. Also, not only is much of our experience beyond our control, but it is such often in ways which do not please us. Sounds scare us, things we see horrify us, or make us weep; things taste bitter, or smell rancid, etc. In short, we have lots of experiences which, if we had been given the chance, we would have skipped.

The conclusion to be drawn from this character of our experience is (P2). That is, we ought to conclude that there is something which exists independently of our experience, and which causes these experiences.

At this point it needs to be stressed that (P2) is really a quite weak claim. (P2) does not assert the existence of the physical objects of common sense, nor the theoretical objects of current science, nor <u>anything else in</u> particular. It merely asserts that there is something, I know not what (so far as (P2) goes), which exists independently of experience. I am now arguing that a plausible basis for that assertion is the fact of our inability to control certain aspects of our experience.

As stated so far, the argument is clearly elliptical, and is also unlikely to be convincing to a non-realist. So what we need to do is bring our unexpressed premises out into the open, and argue for them. Aside from the fact of experience I have appealed to, there are two important unstated

premises in the argument:

1. Experiences which I cannot consciously control are experiences for which I am not the cause.
2. At least some of the experiences which I cannot consciously control have causes.

I will consider each of these premises in turn.

Premise 1 needs to be defended against one important sort of objection: One who wishes to deny the truth of this premise may claim that our lack of conscious control over aspects of our experience does not imply that we ourselves are not the cause of these experiences since we all agree that our own dreams have causes within ourselves. And dreams do have this same characteristic of lack of conscious control which we find in our waking experiences. Thus, it may be claimed that although these experiences do have causes, and although we are not conscious of these causes, the causes are nonetheless internal to ourselves, and may be attributed to our own unconscious, or subconscious, minds.

This objection to premise 1 does not work, because it involves an important misunderstanding of the intent of the premise. The "I" in this premise may not be understood in its most usual sense, i.e. as including a body, a transcendental ego, an unconscious mind, etc. Rather, it must be restricted to the group of experiences which I am currently having. That is, the "I" is here restricted to what we might call the Humean ego.

This restriction is far from arbitrary. The whole

point at issue here is (P2), which claims that something exists independently of experience. Thus if it is to be claimed that nothing but experience exists, then the "I" with which we must start is the experienced ego. And if this reasoning is correct, then the status of any unconscious mind vis-a-vis (P2) is essentially the same as the status of, say, unexperienced physical objects. That is, it is unexperienced or, we may say, theoretical. Most importantly, it is allowed to exist when not experienced. Thus if the anti-realist makes any appeal to an unconscious mind in rejecting premise 1, he has already accepted (P2), which is what the realist was trying to get him to do in the first place.

What remains to be said concerning premis 1 is this: No phenomenalist or solipsist would be likely to claim that cases in which I am conscious of controlling my experiences are not cases in which I cause those experiences. This is so because one who consistently denies (P2) would need to adopt something like a Humean account of causality. After all, as Hume pointed out, all we experience in causal situations is the constant conjunction of the given events, and our own habitual expectations.[1] Thus it is not open to this sort of theorist to deny that cases of consciously-controlled experiences are cases of causal activity.

Our mention of the Humean notion of causality in the last paragraph serves as a transition into a consideration of premise 2. It might be asked: Why should the phenomena-

list accept such a proposition, especially if he has adopted a Humean account of causation? After all, the causal relations referred to in premise 2 all have causes which are themselves unexperienced. But unexperienced items cannot serve as either causes or effects when the causal relation is understood simply as constant conjunction of experiences.

The objector is correct here in asserting that the phenomenalist is not obliged to accept the existence of the causes appealed to in premise 2. Indeed, for the sake of consistency, he had better not. However, it must be stressed here that although the phenomenalist has no such obligation, neither can he give any account of the existence of these "uncaused" experiences. They just are as they are presented, and there is no sort of appeal which may be made to account for why they sometimes display remarkable regularities, and at other times may be disturbingly surprising. By the very fact of his denial of (P2), the phenomenalist must forego any appeals to any unexperienced entities to explain the character of his experiences.

Now it must be granted that this argument is not conclusive as a refutation of phenomenalism. However, the power of the appeals I have been using is indicated by the fact that an important philosopher in the history of phenomenalism, namely George Berkeley, found such arguments convincing. Indeed, in the sense that he agrees with (P2), Berkeley is a realist, although not a consistent one. Not only does Berkeley agree to (P2), but he uses essentially

the argument I've presented here. In fact, I got the argument from him. He puts the argument in the mouth of Philonous in the second Dialogue, and builds on it to argue for the existence of God.[2] Where I have argued only that there is something, I know not what, which is the cause of our experience, Berkeley introduces further premises to argue that that something is God. But at this time I am not concerned with the character of this "something". So far I am only seeking to support the relatively weak principle (P2).

The point of dragging Berkeley into the fray at this point is to indicate that even a well-known (supposed) phenomenalist uses a basically realist argument, and reaches a realist conclusion. Also, the example of Berkeley helps to stress just how non-committal (P2) is.

Now I wish to show that phenomenalism, as I have defined it (the denial of (P2)), collapses into solipsism. The reason for the collapse is this: The key distinction between phenomenalism and realism lies in the former's definition of existence and reality in terms of experience. Experience is what exists, and all statements about reality must be reduced to statements about experiences. The meaning of any theoretical claim is to be exhausted by its implications for experience. The motive for such a move is, often, a quest for certainty.

To be consistent in such a move, surely one must admit that the existence of other people's experience is just as uncertain, or is even more uncertain, than is the

existence of other people's bodies. Indeed, this is the traditional "problem of other minds". A consistently elaborated phenomenalism must reduce statements about other people's experiences to statements about one's own experiences. What is known is taken as more basic here than what is, and we know of other people's experiences only through our own experiences. Thus it is the case that phenomenalism, to be coherent, must require that statements of other's experiences must be reduced to statements about one's own experiences. The percipi which defines esse is one's own percipi. Thus we have solipsism.

The only route out of this is to make a realist move, as does Berkeley. Berkeley does not limit his claim to "esse est percipi", but adds "percipere". That is, he claims that to be is to be perceived or to be a perceiver. To admit into the ontology something which exists independently of being perceived is to adopt (P2). Berkeley does this on two counts. First, he does so by claiming that minds, as unperceived perceivers, exist. It seems that Berkeley introduces minds for purely theoretical reasons, such as the apparent incoherence of having perceptions without perceivers, and the need for some substantive entities to allow for individuals being more than mere bundles of perceptions. Secondly, as I have already mentioned, Berkeley uses a realist argument to argue for the existence of God, another unperceived being.

The reason I have discussed these moves is that I wish to clarify what seems to me to be a key dividing line

in philosophy. Either one allows the existence of what we might call "theoretical entities" (i.e. the sorts of entities (P2) refers to), or one does not, admitting as real only our experience. Once one accepts (P2), the question which remains is: Which theoretical entities exist, and what are their characteristics? Berkeley opts for minds and God. Scientific realists opt for the theoretical entities of science. Although the differences between these positions are great, they share a crucial move: they both accept the truth of (P2). And once this has been done, one may not, consistent with this move, reject the existence of any theoretical entity simply on the basis that we do not experience it. Once (P2) is accepted, it no longer makes sense to try to reduce the meanings of theoretical terms to references to aspects of our experience. Insofar as Berkeley, for instance, continues to attempt such meaning-reductions, and continues to reject entities simply because they are not experienced, after he has presented a realist argument for the existence of God; just so far is he being inconsistent. That is, Berkeley does not claim that God depends for His existence on our experience, nor does Berkeley claim that God depends for His existence on His own experience: As a perceiver, God exists independently of His being experienced. In short, Berkeley's God is a theoretical entity.

Thus, on one side of this line of demarcation (i.e. assent to or denial of (P2)) lies the slippery slope to realism. And as I tried to indicate in earlier paragraphs,

on the other side of the line lies the slippery slope to solipsism.

Before we leave this topic, there is one other sort of objection to my discussion here that ought to be considered. This objection asserts that there is a middle ground between the alternatives of realism and solipsism. This middle ground is allowed for by asserting that there are "possible experiences", or "propensities for experience", or some such entities which provide us with experiences in appropriate circumstances. Now, I claim that "possible experiences" must be treated in either of two general ways, bringing the opponent down on one side or the other of our demarcation line. If, in speaking of these "possible experiences", he is speaking of nothing more than the fact that when we do certain things, we have certain experiences, then he is appealing to nothing beyond our experiences, and is a solipsist.[3] But neither is he offering any explanation of the externality of our experiences. If, on the other hand, he uses the term "possible experience" in such a way as to support subjunctive conditionals, then he is implicitly appealing to something which exists unperceived. That no-one has succeeded in reducing the content of such conditionals to statements about our experiences alone is well-known.[4] And that no-one has succeeded in this task is due to the fact that these conditionals involve the implicit claim that there is something which remains in existence while we do not experience it, such that <u>if</u> we <u>had</u> experienced <u>it</u>, our

experience would be of such-and-such a character. This theory of subjunctive conditionals will be developed more fully in Chapters 5 and 6.

I hope the discussion of the last several paragraphs has made it clear that there is a wide range of philosophic positions which fall on the realist side of our demarcation line. Indeed, it appears that only the most rigorous solipsists can deny realism in the sense it is demarcated here.

It may now be asked: If the acceptance of (P2) is so pervasive, how could its acceptance be of any significance? The answer is this: Once (P2) is accepted, consistency demands that one adopt realism in its full force. All those who accept (P2) thereby agree that existence is not to be defined by reference to experience, since entities are allowed to exist independently of being experienced. Also, if (P2) is accepted for any reason similar to that given in my argument for (P2) (i.e. that our experience seems to have an external source), then the requirement is adopted that the postulation of the theoretical entities should serve to allow us to explain the character of our experience. Once we have this basis of agreement, then the remaining metaphysical issue is:

(Q3) Which set of postulated theoretical entities best accounts for the character of our experience?

Clearly, any answer to (Q3) involves some projection: claims are made which project beyond the content of our experience.

This raises, of course, the traditional problems of induction--i.e. the problems involved in justifying the making of these projections. These problems will be briefly discussed in Chapter 5.

NO FOUNDATIONS

Once it has been established that our task is to seek the theoretical account which best explains the character of our experience and that such an account must involve appeals to theoretical entities, then the question naturally arises as to whether or not there are any indubitable claims which could serve as a foundation upon which to construct our system of knowledge. I wish to argue here that there is no such foundation. This claim can be expressed in this manner:

(P3) There is no foundation of knowledge. That is, there are no knowledge-claims which we can know a priori that we know; or at least there are no such claims which are sufficient for the derivation of a significant system of knowledge.

Let me try to clarify this claim. It is not a denial that we have a priori knowledge, at least if the latter is understood in the minimal sense of innate true belief. It is quite possible that we have such beliefs, or that at least we have innate beliefs which are pretty good approximations to truth. However, such claims cannot serve as foundations

for a system of knowledge. There are at least two reasons for this. First of all, in most cases we cannot be said to know that we know them, or at least in most cases we cannot be said to have good reasons for believing that they are innate true beliefs. Indeed, I will show that in some cases, our innate beliefs have turned out to be false.

The second reason why innate true beliefs cannot serve as foundations for knowledge is that in cases where we could perhaps be said to know that we know them (e.g. the claim that there are experiences), such claims are nowhere near sufficient to serve as the basis of derivation of a significant system of knowledge.

Why, it may be asked, must foundational knowledge-claims be ones which we know a priori that we know them? Well, the a priori part of this demand arises because a foundation of knowledge would, presumably, be that from which we start the construction of our system of knowledge, and if it were not a priori, it would involve appeal to some evidence, and would hence not be the starting point of the system of knowledge. On the other hand, we must know that we know the claim, or at least have true belief that we know the claim. If we did not believe that we knew it, we would not desire to use it as a foundation, and if it were not a true belief, it would not be a foundation for knowledge (since it would be false to say that we know the foundation to be true).

In this context, true belief that we know the truth of the foundation is all that can be required, since if we

were to claim that this true belief (if it existed) must be justified, then we would need to justify it by appeal to some evidence, and the foundation would then no longer be our starting point, and would hence not be a foundation.

The truth of (P3) can be given some further support by simply noting that neither epistemology nor metaphysics may claim priority in philosophy. We can put the point this way: To make any epistemic claims, we must always assume some part of a metaphysics. And to make any metaphysical claims, we must always assume some part of an epistemology. Consider, for example, a phenomenalist epistemology, which claims that the basic knowledge claims which are to serve as evidence are reports of immediate sensations. Such a claim includes the metaphysical assumption that there are such entities as immediate sensations, and reports of them. But if we turn to metaphysics, and try to justify the commitment to such things as immediate sensations and reports, then we must have standards of evidence. But the phenomenalist standard of evidence presented here is one which presumes the existence of the very entities we are setting out to justify belief in. Phenomenalists are not alone in this dilemma. The same point can be made, I believe, with respect to any philosophic system.

The only way out of these difficulties would be to claim that belief in the existence of some particular entities does not require the support of evidence. That is, one must say that the claims for the existence of such entities

are self-evident, if one is to have hope of maintaining a foundationalist position. Now it seems reasonable to suppose that if a claim is truly self-evident, then it should be so to anyone who understands it adequately. And to prevent begging the question, the criteria of adequacy of understanding ought not include recognition of self-evidence. I, for one, have yet to find any significant claim which I am willing to acknowledge as obviously self-evident. And to provide some support for the view that there are no such claims, I will show, with several examples, that some claims, which have been said to be self-evident, are not such.

1. Consider this axiom of Euclid: "Given two straight lines, they will meet in at most one point." This axiom was a statement for which self-evidence was often claimed, at least prior to the 19th century. And, recalling that intuition has often been used as a basis for claiming self-evidence, we may note that our intuitions do indeed lead us to assent to the truth of this axiom. This sort of intuition is what I have been referring to as "innate belief."

But the development of non-Euclidian geometries opened up the possibility that this axiom may be false. Indeed, in an elliptical geometry, two straight lines cross twice. And if it should be the case that the geometry of our universe is elliptical, then the axiom of Euclid, stated above, is false. A statement which is thus dependent on contingency for its truth cannot be said to be self-

evidently true.

2. Another example, or set of examples, is drawn from the foundational studies of quantum mechanics. Here are two examples:

(a) p or (q and r) ≡ (p or q) and (p or r)

(b) (p or q) is true ≡ (p is true) or (q is true)

Each of these has been at one time or another claimed to be self-evident truths of logic. But if quantum mechanics is true, and if the quantum-logic interpretations of that theory are correct, then both of these propositions are false. Of course, quantum mechanics may be false, and in any case, the quantum-logical interpretations of the theory may be false, but that does not detract from the point being made here. The mere fact that there is this theory, and there are competent people who seriously propose the quantum-logic interpretations, shows that (a) and (b) cannot serve as examples of self-evident truths.

3. We have so far briefly considered two candidates for self-evident truths which have been of the sort referred to as a priori. Now I wish to consider a candidate for the status of empirical self-evident truth. The usual candidates for such status are reports of immediate experience, which include no commitment to the existence of objects. We can try to express such a candidate by a statement such as: "Red is appearing now". This is about as non-committal a statement as I can think of, which still makes an assertion with some content. Also, it is very simple: We can imagine

it to be the report of a visual experience where the entire visual field is filled by a homogeneous red expanse.

Many theorists have claimed that such minimal reports are self-evidently true. They claim that the reporter could not make a mistake, and that there is no logical possibility of error, so long as he knows how to use the required terms. And it has been claimed that the certainty has this source: The report does not go beyond the immediate experience. In other words, the report involves no projections. Hence there is no room for error to creep in.

I wish to now present an argument to the effect that there is conceivable uncertainty even in such a non-committal report. Room for uncertainty appears to enter in this way: Any report of experience involves projection beyond the content of the immediate experience. Such projection enters through the use of descriptive terms, e.g. "red". To report that one is experiencing red is to report that one is having an experience that is appropriately similar to certain experiences one has had in the past, and might expect to have in the future. I am not concerned here with projection to future instances only--I am concerned with projection beyond the immediate experience.[5] And past experiences are not, of course, part of the immediate experience. The most presence which past experiences can achieve occurs via memory. But it is virtually a truism that memory cannot be relied on for the sort of certainty philosophers have sought.

Later in the thesis I will argue that we cannot be

said to be mistaken in such cases, or at least in cases which are similar to those of this argument. However, those arguments will involve fairly high-level theoretical considerations. My point here is that such reports cannot be said, a priori, to be self-evident, because arguments like the one I just presented can be seriously held.[6] I am arguing, in general, that claims of certainty, or claims of impossibility of error, are themselves high-level theoretical assertions, and hence cannot serve as foundations (starting-points) for systems of knowledge. Such an argument is not inconsistent with the assertion that there are some situations in which, as a matter of fact, we cannot be mistaken.

Thus I claim that we do not have a priori certainty in any of our knowledge claims. Once one accepts this position, then it becomes clear that knowledge cannot have any foundations. Foundations of knowledge must be a priori indubitable. Once they are doubted, then they can be argued for or against, and other beliefs may be brought to bear in deciding their truth. When this is done, they are no longer serving as foundations, or starting points, for all knowledge.

If the arguments I have presented so far have succeeded in establishing their conclusions, then we now have two key truths about the task of seeking truth: First of all, we are to seek an account of reality, making full use of theoretical entities without any apologies, which best accounts for the character of our experience. This task was expressed in (Q3). Secondly, we are to embark on this task

without any a priori claims that some entities indubitably exist, and hence must be involved in any theoretical account of reality. This is the content of (P3). This means that all metaphysical claims start off with equal status--all require the support of evidence and arguments.

For instance: It follows from the above conclusions that the task of metaphysics is not to start from immediate sense-experiences, and construct from them, or even infer from them, the nature of what lies 'behind' them. Rather, the task of metaphysics is to ascertain the nature of reality, which may or may not include such items as "immediate sense-experiences." It is true that in much of my arguing I have tacitly referred to such. But as I mentioned at the beginning of this inquiry, my activity has not been one of establishing a foundation for knowledge. Rather, I have been seeking to convince those with phenomenalistic convictions, and to do so, I have started from assumptions I believe they might accept.

It will turn out that I will allow that there are immediate-sense-experiences, and even that there may be a special language that may be used to report their character. That I have such beliefs is what allowed me to argue from a basis of agreement with phenomenalists. But a key difference between my system, and that of most phenomenalists, is that the latter treat the characteristics of immediate sense-experience as a priori indubitable. But in the system being developed here, our knowledge that percepts exist is

the result of high-level theoretical considerations, and hence cannot serve as a foundation for knowledge. But more on that topic later.

THE REALITY OF KINDS

A very general feature of any system of philosophy is whether or not it includes universals among its fundamental entities. That is, is the system realist vis-a-vis universals, or is it nominalist or conceptualist? In later sections of this thesis, arguments will be developed which depend on the claim that kinds (universals) are real. Therefore, in this section, I will attempt to provide some reasons for believing that universals are, indeed, real. Most of the reasons presented here will be negative, in that they will be arguments against nominalism and, to a lesser extent, conceptualism.

Before we proceed to those arguments, we need some sort of characterization of the issue. Panayot Butchvarov portrays the problem as one of "the proper description of the recurrence of qualities."[7] With the proviso that a description, intended as such, be understood as involving one in ontological commitments, I can accept this portrayal of the issue. Nominalism, then (in one form), may be understood as the claim that the recurrence of qualities is no more than the recurrence of terms. Nominalism in a second form may be understood as the claim that recurrences of qualities are just the occurences of members of a set. Nominalism in a third form (which Butchvarov refers to as the resemblance theory) claims that "the instances of a recurrent quality are dis-

tinct particular qualities related by a relation of resemblance."[8] What I take to be common to all forms of nominalism is the claim that one may not properly refer to recurrent qualities as being the same quality, which is, I take it, the claim that there are no such entities as kinds (universals). Conceptualism is, then, a form of realism, in that it admits that there are such entities as kinds, but it claims that these are mental entities. The sort of realism I seek to defend claims that there are kinds, and that they exist independently of minds. I shall now proceed to a criticism of the opponents of this form of realism.

Nominalism

We can begin by considering the first form of nominalism, which claims that kinds (types) are merely linguistic. That is, it claims that two particulars are of the same kind (type) only in so far as the same term is applied to both. In other words, it claims that there are no universals for particulars to share--they share only applications of terms in languages.

There are two parts to my argument that this form of nominalism is incoherent: (1) This form of nominalism requires the reality of linguistic kinds; and (2) Linguistic kinds are a subset of the natural kinds. I will argue these points in turn.

(1) This form of nominalism makes the claim that various particulars are referred to by the same term. Now

if what was meant by "same term" was "same term-token," then this claim would be clearly false. For instance, if I say: "This apple is red", and then say: "That apple is red", the "red" in each of these sentences is not the same token (i.e. is not the same particular). Rather, they are the same term-type--i.e. they are the same kind of term. And given that terms are linguistic entities, term-types are linguistic types (kinds).

If the nominalist refuses to admit these linguistic kinds, then he is forced into a vicious infinite regress. He must say that the two term-tokens of "red" mentioned above are the same type only because the same term is used to refer to them at the meta-linguistic level. The problem then recurs at the meta-linguistic level, so he must then retreat to a still higher level, etc. ad infinitum.

(2) Term-tokens are always something like a set of marks on paper or a series of noises, etc. So for two term-tokens to have the similarities required for them to be of the same term-type, the mark-tokens on the paper-tokens, or the sound-tokens, etc. must have the appropriate similarities. That is, the marks on the paper, or the sounds, etc. must be of the same type. But these types are examples of intrinsic, or natural, kinds. And if the nominalist tries to explain these away by saying they are no more than applications of the same term, he falls into the difficulties of (1). Thus it is that by trying to explain away natural kinds by reference to terms, nominalists succeed in explaining away

nothing, since for their account to have any hope of success, it must make use of the very entities it seeks to explain away.

The second form of nominalism we are to consider claims that recurrences of qualities are all members of a set, and that the kind is that set. One of the most striking problems for this theory of kinds (I am not claiming that it is the only problem) occurs with kinds that do not happen to have any token occurrences, such as non-human featherless bipeds, mechanical persons, and cures for cancer. Nominalists who identify kinds with sets must claim that each of these is the same kind, since each of them is the same set, viz. the empty set. It is, I submit, absurd to claim that mechanical-person-hood is the same kind of entity as is cure-for-cancer-hood. If they were the same kind, then surely researchers who hope to construct one would follow the same methods as those who seek to construct the other. Hence kinds cannot be identified with sets.[9]

The third form of nominalism, which may be referred to as the resemblance theory, has been soundly criticized by Bertrand Russell:

> If we wish to avoid the universals whiteness and triangularity, we shall choose some particular patch of white or some particular triangle, and say that anything is white or a triangle if it has the right sort of resemblance to our chosen particular. But then the resemblance required will have to be a universal. Since there are many white things, the resemblance must hold between many pairs of particular white things; and this is the characteristic of a universal. It will be useless to say that there is a different resemblance for each pair, for

> then we shall have to say that these resemblances resemble each other, and thus at last we shall be forced to admit resemblance as a universal.[10]

The alternative to accepting the final consequence of Russell's argument is to retreat down an infinite regress of resemblances of resemblances of resemblances.[11]

The inadequacy of all three forms of nominalism can be seen from another direction, which is epistemological rather than metaphysical. How could nominalists choose what is to count as a particular among the phenomena of sense-experience, and how could they defend any such choice? I contend that the picking out of particulars in experience requires that qualities be experienced as kinds of qualities. According to the first sort of nominalism, our visual experiences are composed of patches of colour, which are categorized as to type by our linguistic conventions. But if the colour types are no more than linguistic conventions, then so also are the particulars no more than linguistic conventions, since we distinguish particulars by means of observing borders between colour-types! The second sort of nominalism runs into the same sort of problem in terms of the construction of its sets. The sets have particulars as members. But if particulars are distinguished as such by the kinds of properties they have, then this requires an awareness of the kinds (sets) prior to any selection of particulars which could serve as members of the sets! And this difficulty confronts the resemblance theory by requiring that the awareness of resemblances be prior to an aware-

ness of the particulars (as being particulars) which are required to serve as the relata of the resemblance relations.

The lesson of these last arguments is that the token-type (universal-particular) dichotomy constitutes a complementary pair. If one claims that types are conventional (viz. that there are no natural kinds) one must also claim that tokens are conventional (viz. that there are no natural particulars). Nominalism, by claiming that only one half of the complementary pair exists, renders itself inescapably incoherent.

Thus we see that there is a need to adopt some form of realism vis-a-vis kinds or universals. Once we have demonstrated the need for some kinds, the question is no longer one of whether or not to allow kinds into the ontology, but which kinds to allow in. I shall argue that this further question can be answered only by empirical means.

It should be emphasized that the above arguments did not, and were not intended to, support Platonism, if we construe the latter as the view that there are special individual entities which are the universals. This is the "perfect particular" version of Platonism. By treating universals as though they were a special sort of particular, this position gets into troubles which are similar to those confronted by nominalism. The best known of such troubles is usually referred to as the problem of the third man. The problem here is that if two men can be said to be the same kind of entity (i.e. they are both men) because they

both "partake in", or resemble, or have some special relation to the Platonic perfect particular "manhood", then we can raise the question: In what does the similarity of, say, the first man, and manhood, consist? It seems, then, that yet another perfect particular, a "third man", must be appealed to, and so on, ad infinitum. This problem is, I take it, analogous to the infinite regress of resemblances fallen into by the resemblance theory, and is also analogous to the infinite regress of term-tokens fallen into by the first sort of nominalism considered here. Such infinite regresses are, I submit, a hazard common to all attempts to reduce universals to particulars, in whatever way this reduction may be attempted.

Conceptualism

Now that we have seen the incoherence of nominalism, it remains for us to look at conceptualism. Conceptualism may be characterized as the view that objects which are said to be of the same kind are such only because we apply the same concept to them. Now, this allows that there are real kinds (hence conceptualism is a variant of realism), but it limits real kinds to mental kinds.

I wish to argue that conceptualism shares with nominalism a radical conventionalism, since they both imply that the ordering of our experience is imposed on it by us. For the nominalist (at least of the first type), this is done by applying terms to our experience. For the conceptualist, it is done by applying concepts (mental kinds) to

our experience. Normally, conceptualists restrict this conventionalism in various ways. For instance, the conventionalism may be relativized to the entire human race, in that the concepts would be innate in all members of the human species. I understand the Kantian position to be something like this. Or, the conventionalism may be relativized to conceptual systems, which may be shared by large numbers of individuals. In either this case or that of the Kantian, an individual's ability to alter his conceptions may be somewhat limited--he will not be able to categorize his experience in any arbitrary way.

Regardless of these restrictions, it is still the case that reality is depicted as placing no restrictions on the manner in which it may be conceptualized. The only restrictions are those of the mind, be they inherent in the species or trained in from an early age.

It should be noted here that if conceptualism is combined with idealism, so that reality is regarded as being nothing more than minds and their contents, then conceptualism collapses into realism. That is, the real mental kinds (concepts) encompass all of reality. Now this should not be too surprising, since conceptualism is a form of realism --it just restricts the natural kinds to mental kinds. So if reality is then restricted to the mental, the realism of conceptualism is complete. But in any case, this does not remove the conventionalist content of conceptualism. Conceptualism implies the view that the mind is the source of

all regularity: There is no regularity--no sorting as to kinds--except that imposed by the mind. And this latter view is radical conventionalism.

The problem with all of this is that it apparently leaves no room for there to be error. That is, in a radical conventionalist scheme, what is it that distinguishes being correct from being in error? An apparently easy answer is this: The distinction between truth and error is itself a convention. And this claim might be supported by an argument such as this: An important type of 'error' is that represented by visual illusions. For instance, a stick looks bent in water, while out of the water it looks straight. Or, in the desert we see a lake, but as we approach the lake it disappears. Or, a surface looks white; but when we look at it through a microscope, we see that it is composed of a multitude of variously coloured dots. In each of these pairs, the first description is said to be "mistaken", and the second is said to be "correct". Clearly, it is argued, this is a convention. We have selectively chosen some of our experiences to be labelled as 'illusory', and we have chosen others to be labelled as 'real'. But they are all, equally, genuine experiences.

This argument misses the thrust of the truth-error distinction. Consider the stick in the water that appears bent. The contrast between illusion and reality is not a contrast between experiences. It is, rather, a contrast between ways of reporting experiences. The error occurs in

reporting: "The stick is bent". To say the stick is bent implies that if one feels it, it will feel bent; or when pulled out of the water, it will still look bent. It is implicit predictions such as these, which are present in any report or description of experience, which provide the basis for the realist distinction between being correct or being in error. Such predictions express our expectations, and these expectations can clash with our later experience. We expect the stick to feel bent, but it does not. Rather, it feels straight. Or, we expect the stick to look bent when pulled out of the water, but the bend moves down the stick as the stick is pulled from the water, and is gone when the stick is out of the water. Similarly, we expect the lake in the desert to remain in sight as we approach it, and to quench our thirst and cool our feet. But our expectations are frustrated. Our experiences are quite contrary to the expected ones. Most importantly, our experiences are of the wrong kinds. The kind of experience is different from the expected kind of experience. (More will be said on this topic when I develop a theory of error in Chapter 5). I see no way in which a conventionalist could explain how we could have experiences of a kind which are contrary to the expected kind. If we are the sources of the kinds of experiences, then one would expect that this source would never categorize contrary to its own expectations. Surely we would categorize in such a way as to meet our expectations. I can grant that some experiences offer enough room for

categorization more in line with expectations, but the point here is that there are real limits to the abilities of varied interpretations to meet expectations.

Finally, we may note that the conventionalist may try to answer these objections by pointing out that any specific claim can be maintained in the face of any and all evidence, provided only that suitable variations are introduced elsewhere in the theoretic system. That such can be done in many cases is a point with which I agree. But this fact will not work as a defense of conventionalism. The point is that changes in some part of the theoretic system is forced on us. If radical conventionalism were true, no change could ever be forced on us.

Thus it is evident that any form of radical conventionalism, be it nominalist or conceptualist, cannot handle, in any adequate manner, the fact that our experiences can, and do, run contrary to our expectations. For this reason, and the others presented earlier (contra nominalism), I claim that there are real natural kinds.

Positive Account

The question that naturally arises at this point is: What positive account of universals can be offered? I have, as yet, been unable to develop a positive account of universals which I find wholly satisfactory. However, we should at least be prepared to accept the possibility that no positive account of universals can be given. That is, we may have to adopt kinds as unexplained explainers in any adequate

philosophic system. That this might be the case was indicated by two aspects of the preceding discussion: (1) attempts to reduce universals to particulars (as in linguistic nominalism, resemblance theory, or "perfect particular" Platonism) all appear to run into the same problem of infinite regression of particulars; and (2) discrimination of particulars requires a prior awareness of the kinds of experience.

I am suggesting that it might be the case that universals and particulars form a complementary pair, such that any account of reality cannot avoid appeals to either, and such that any attempts to reduce one to the other are doomed to failure. In any case, in the remainder of the thesis I will assume that at least some kinds are real, and I will repeatedly appeal to their reality to aid in explaining other features of reality.

Chapter 3

METAPHYSICS

In this part of the thesis, I shall begin to develop some key metaphysical doctrines that will be used in solving the problem of secondary qualities. The presuppositions argued for in the previous chapter will be used uncritically. In particular, I shall assume that we are seeking a characterization of reality as it is in itself, apart from any consideration of whether it is being experienced or not. I shall seek this characterization without attempting to base it on any *a priori* indubitable foundations, and I shall unhesitatingly appeal to *kinds* of entities whenever it is necessary.

INTRINSIC AND RELATIONAL PROPERTIES

A distinction of long standing in metaphysics is that between intrinsic properties and relational properties. Corresponding to this distinction is the distinction between unary predicates, on the one hand, and binary or higher n-ary predicates, on the other. The predicates *refer* to property-kinds. However, it should be noted that the referent of a predicate does not exhaust its meaning, since the meaning of a term is its *use*, and terms have other uses besides referring. (For further elaboration of this notion of meaning, see Chapter 6).

But our main concern here is with ontology rather than with semantics, so I will be discussing properties rather than predicates. A convention which will be used throughout this discussion should be made explicit from the outset: Whenever I refer to properties, I am referring to property-kinds, unless otherwise stated. The reality of property-kinds was, I hope, established in the last chapter.

In discussing the distinction between relational properties and intrinsic properties, we will need some sort of criterion by which we can distinguish them. I propose the following criterion for that purpose:

(P4) Intrinsic properties are those which could be meaningfully attributed to a particular even if that particular were all that existed (i.e. if it were the universe). All other properties are relational.

This criterion is obviously not an empirical criterion. Rather, it is conceptual. In order to determine whether a property is intrinsic or relational, it asks that one perform a thought-experiment.

A key point is then this: Structural properties are intrinsic properties. There is a mistaken tendency to identify structures with relations, which is due to the fact that structures are resolvable into sets of relations among particulars. To clarify this claim, I will now discuss a simple model.

The structure we will consider is a simple geometric structure ("geometric" because its relations are spatial) composed of 5 particulars (named U, L, C, R, and D) and having four primitive relations ("above", "below", "to the right of", and "to the left of"). The particulars have the relations depicted in Figure 1.

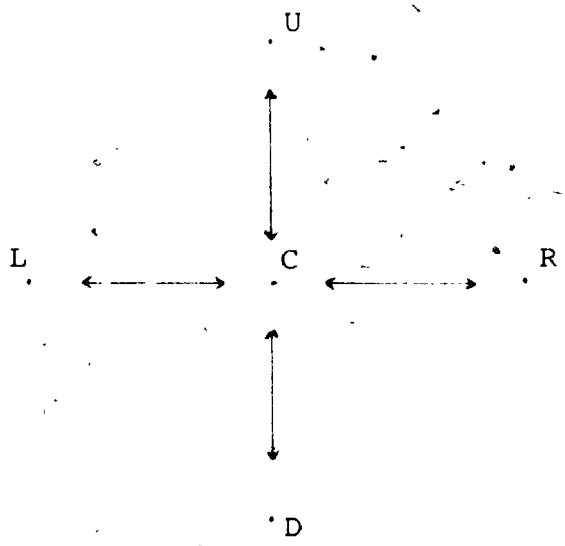


Figure 1

An Illustrative Structure

This whole model can be regarded as a particular (name it "V"). It is this particular which has the structure we've depicted in Figure 1. We could use the term "cross" to refer to the structural property. (The term "structure" is ambiguous between the particular V and the cross property). That this structural property is an intrinsic property of V is indicated by the fact that it is meaningful to attribute the cross structure to V even if V were the entire universe.

With the names and relational predicates we have introduced so far, we can describe V. For instance, we can

say: "R is to the right of C", "U is above D", "D is below and to the right of L", "No particular is above U", etc. Note, however, that the particulars U, L, C, R, and D cannot be described, except by giving their relations to the other particulars (including V). For instance, we could describe C in this manner: "C is the particular which is above D, below U, to the left of R, to the right of L, and part of V".

The difference between the manner in which we described V and the manner in which we described C is quite significant, and will have important consequences when we discuss the problem of secondary qualities. The type of description we gave for V will be referred to as an intrinsic description, while the type of description we gave for C will be referred to as an extrinsic description.

In the model as I have presented it so far, only V has any intrinsic properties, and that is a structural property. The particulars U, L, C, R, and D have been depicted as having no properties but relations. But suppose that C, itself, has a cross structure, so that what C is is a set of five particulars, call them C_u, C_l, C_c, C_r, and C_d, which have the same relations among them as do U, L, C, R, and D to each other. C then has an intrinsic property which is, in this case, a structure. We can then describe C both extrinsically (by depicting its relations to U, L, R, D, and V), and intrinsically, (by depicting the relations among the particulars which make up the structure).

I trust that the way in which structures lend them-

selves to both extrinsic and intrinsic descriptions has been made fairly clear by consideration of this simple model. But one problem with the example which we have developed here is that it displays intrinsic and extrinsic descriptions of particulars, while what I am really after here is the display of intrinsic and extrinsic descriptions of properties. However, this problem is not at all serious. Although we have provided examples of description of structural particulars (i.e. V and C), the description of structural properties is not very different in principle. For instance, to provide an intrinsic description of the structural property held by both V and C (i.e. the cross structure), one need only use variables instead of references to particulars. Thus the description of the cross structure, as a kind of structure (a structural property), might proceed in this manner: The cross structure is composed of any 5 particulars, and the four relations "above", "below", "to the right of", and "to the left of", such that one of the particulars is above all others, another is below all others, a third is to the left of all others, a fourth is to the right of all others, and the fifth is below the first, above the second, to the right of the third, and to the left of the fourth.

When we consider how one may provide an extrinsic description of a property, procedures change somewhat. An extrinsic description of a property does not provide the extrinsic spatial relations of a property, since a property does not have such relations--only particulars have such

relations. However, extrinsic descriptions of properties can be given by displaying their relations to other properties in some abstract space. For instance, the cross structure may be said to have the relation "fewer vertices than" to any type of structure which requires more vertices in its description (e.g. a cubical structure requires eight vertices). But although there is this difference between extrinsic descriptions of particulars and extrinsic descriptions of properties (i.e. the former can occur in concrete space, while the latter must occur in some abstract space), they do share the essential feature of stating relations to other entities.

Now I wish to assert a key point which will find important application in solving the problem of secondary qualities:

(P5) Intrinsic descriptions are possible only for structures. Neither relations nor non-structural intrinsic properties can be described intrinsically.

Thus I am claiming that while structural properties may be described either intrinsically or extrinsically, descriptions of simple relations and non-structural intrinsic properties are restricted to extrinsic descriptions.

The truth of (P5) is readily seen by considering just what is involved in describing anything. When we describe, say, a person's appearance, we provide skin and hair colour, size and relation of eyes, ears, nose, mouth, etc.

Such description is possible simply because the person's appearance is a complex arrangement of colours, shapes, etc. Similarly, when we describe a person's character, we relate how he reacts in various kinds of situations. What is common to such descriptions, and indeed what is common to all descriptions, is reference to a complex of relations, viz. a structure. When such a complexity is absent, one is at a loss for description. And the only complexities we can appeal to are either internal to that which is being described (in which case we are providing an intrinsic description of a structure) or are external to it (in which case we are providing an extrinsic description).

More support for (P5) is found by considering just what moves are available in attempting to describe simple relations and non-structural intrinsic properties. Consider first the simple phenomenal relation "above" by providing examples: e.g. "The ceiling of a room is above its floor". However, such does not constitute a description of "above". It is, rather, an exemplification of the relation. The claim of (P5) is that the best we can accomplish by way of describing the phenomenal relation "above" is to say something like: "Above" is the relation opposite in direction to "below", the pair of which are perpendicular to the pair "left" and "right". Such a description is extrinsic, since it indicates how "above" is related to other relations.

When we attempt to describe non-structural intrinsic properties, we are confronted by a similar situation: either

we simply exemplify the property, or we describe it extrinsically. Phenomenal colours serve as good examples of non-structural intrinsic properties. Indeed, the fact that one cannot provide intrinsic descriptions of colours is notorious: e.g. one cannot describe the difference between red and green to someone who is colour-blind. The best one can do is provide extrinsic descriptions. For instance: "Red is the colour which, when combined with yellow, produces orange," or "Green is the colour which results from combining blue and yellow." These are extrinsic descriptions, because they indicate the relations one colour has to others.

I presume that these examples have at least offered some clarification of the distinction between intrinsic and extrinsic descriptions. In any case, further examples will be forthcoming throughout this thesis.

Before moving from the topic of the description of properties, more needs to be said concerning the exemplification process. I have stressed that such is <u>not</u> description, although it must be admitted that like description, exemplification is an important means of conveying the meaning of a term. But it differs from description in its peculiarly <u>ostensive</u> character. Where exemplification is most ostensive, i.e. when one points to an example within range of the senses of the subject, then exemplification serves to <u>acquaint</u> the subject with the property or object in question. What we have here is, of course, essentially the distinction Russell drew between "knowledge by acquain-

tance" and "knowledge by description".[12] Although I am rather hesitant in applying the honorific "knowledge" to the former, the distinction I am making here between exemplification and description does amount to a distinction between conveying knowledge by acquaintance and conveying knowledge by description.

Finally, it must be stressed that whereas one cannot intrinsically describe relations and non-structural intrinsic properties, one can become acquainted with them. The difference between someone who is colour-blind and someone who is not is that the former is acquainted with the properties red and green, while the latter is not. It is this acquaintance with properties which allows any descriptions to occur. To understand either an intrinsic or extrinsic description of any property, one must be acquainted with the relations appealed to in the descriptions. More will be said of this acquaintance in Chapter 5.

STRUCTURAL METAPHYSICS

The distinctions we have drawn in the last section can be of some help in delineating the traditional distinction between form and content. There is a strong tendency, I believe, to equate form with structure and relations, and to equate content with intrinsic properties. But note that if I am correct in claiming that structures are intrinsic properties, then this way of distinguishing between form

and content breaks down. This observation leads me to equate the content of the form-content dichotomy with non-structural intrinsic properties. Once we draw the distinction in this way, it is easy to delineate three different types of metaphysics, depending on whether or not contents are admitted into the ontology, and depending on the role played by structures in the metaphysics. These three types of metaphysics can be designated as bare particular metaphysics, infinite hierarchy of forms metaphysics, and form-and-content metaphysics. I shall discuss the first two only very briefly, and will develop the rest of the thesis in the context of the third.

Bare Particular Metaphysics

This type of metaphysics denies the existence of non-structural intrinsic properties, admitting only structures and relations of which they are constructed. The fundamental relata are then nothing more than bare particulars.

At this point I wish to indicate some misgivings over this type of metaphysics. I do not claim that these misgivings constitute conclusive reasons for rejecting this sort of metaphysics, but I do claim that they serve to indicate possible problems which its supporters ought to consider. My reason for rejecting this type of metaphysics is constituted by my reason for accepting the form-and-content type of metaphysics, which is provided when I consider

that metaphysic.

One misgiving I have over an appeal to bare particulars is that it is not clear that they can properly be distinguished from mere nothingness. This indistinguishability from nothingness would be obvious if bare particulars, besides being denied to have intrinsic properties, were also denied to have any relations. But of course they are usually used to serve as the relata for relations. But still, it is not clear how this entity which is serving as a relatum is itself distinguishable from nothingness. After all, shouldn't it be distinguishable prior to serving as a relatum?

A point similar to the last one can be put this way: Supposing the relations at issue are spatial relations, then how are these relata to be distinguished (ontologically, not merely epistemically) from empty space?' If bare particulars have no properties but their spatial relations, then how do the bare particulars differ from an arbitrary position in empty space? Such an arbitrary position has plenty of spatial relations. If it is answered that the bare particulars can move, this is not helpful in clarifying what it is that is being referred to when it is said to move.

Another misgiving I have with regard to bare particulars is that it seems that to even begin to make sense of current physical theory, we would have to introduce several different kinds of bare particulars, such that when placed in certain relations to other bare particulars, they would behave differently. My misgiving here is based on a convic-

tion that in order to claim that two substances are of different kinds, one must claim that the substances have distinct kinds of properties. But such could not, of course, be said of bare particulars, as they have no properties.

Infinite Hierarchy of Structures Metaphysics

This type of metaphysics, like the bare particulars metaphysics, could be classified as a metaphysics of pure form, since it also involves the denial of non-structural intrinsic properties. In this type of metaphysics, there is an infinite progression of levels, so that the particulars of any given level, although they do have intrinsic properties, have only structural intrinsic properties. Thus the particulars at any given level are resolvable into sets of relations among the particulars at the next level down.

The most serious misgiving I have with respect to this type of metaphysics is similar to one I expressed with regard to the bare particular metaphysics: How, in such a system, are matter and empty space to be distinguished? Consider the fact that in any region of empty space, one can pick out a structure, and even an unending hierarchy of structures (provided the space is continuous), where all relations are spatial, and all particulars are structures in space further resolvable into spatial relations among still smaller structures. What is there, in a metaphysics such as this, to distinguish between such structures and the material structures?

If it is answered that the material structures move, while empty space does not, the same problem recurs with regard to distinguishing between arbitrarily chosen sequences of spatial structures and the material structures. The arbitrary sequences of structures can be selected so as to simulate motion by choosing at successive times structures slightly displaced from previously chosen structures. Once again, in such a metaphysics, the arbitrary structures chosen in empty space are metaphysically indistinguishable from the so-called material structures. In short, matter cannot be distinguished from empty space in such a system.[13]

Form-and-Content Metaphysics

This type of metaphysics allows that there also exist, besides structures and relations, non-structural intrinsic properties. This type of metaphysics does not fall prey to the difficulties encountered by the others. First of all, particulars in such a scheme are not in danger of being indistinguishable from nothingness, since they are always composed of tokens of some intrinsic properties. Secondly, there is no difficulty in distinguishing matter from empty space, because matter has certain non-structural intrinsic properties (e.g. mass, charge) which are not possessed by empty space. And thirdly, particulars can readily be sorted into kinds by virtue of their having different intrinsic properties.

But aside from having these advantages, the form-and-content type of metaphysics has a general advantage at a

more systematic level. While I have thus far displayed difficulties for the other types of metaphysics, and have shown that the form-and-content type of metaphysics does not fall prey to those difficulties, there is a type of argument which strongly favors the form-and-content type of metaphysics. That argument is this: The form-and-content metaphysics is an integral part of theoretic systems which have been empirically confirmed. That is, such a metaphysics is a tacit presupposition of current physical theories. That this is so is shown by the fact that current physical theories do introduce non-structural intrinsic properties, and use them to explain physical interactions in reality. This assertion will be filled out in more detail in the next section.[14]

Finally, let me stress that the argument for adopting form-and-content metaphysics is, ultimately, the claim that it is part of a system which is more adequate in accounting for our experiences than are competing systems which contain either of the other types of metaphysics. Indeed, in our discussion of the secondary qualities, the role this type of metaphysics plays in the solution to that problem will be important, so that by the fact that this metaphysic allows for a physicalist solution to the problem of secondary qualities, we will have a strong argument in favour of it. And as I have noted before, this argument in favor of the form-and-content type of metaphysics serves as a strong argument against the adoption of either of the other types of

metaphysics.

STRUCTURAL METAPHYSICS IN MATHEMATICS AND PHYSICAL SCIENCE

In order to more fully illustrate the claims made in the last section, and in order to develop some important points which will be of use in solving the problem of secondary qualities, I will now take a brief look at the role structures play in mathematics and the physical sciences. In exploring the roles played by structures in these fields, I will try to indicate the relevance of the three types of metaphysics I outlined to the claims made in these fields. Also, we will want to keep an eye on which types of descriptions are used at any given point.

Mathematics

Mathematics deals with abstract, or possible, structures. At this time, I will not try to answer the question as to what is the ontic status of possible structures. But we can note that asking what possible structures are seems like an odd question. It sounds like asking what, in actuality, are possible structures. But an important feature distinguishing possible structures from actual structures is that the former are often not actual. The problem then is that this feature does not serve to distinguish them from the impossible structures, which are also not actual.

It has often been said that mathematics has no content.[15] Another way of saying this is to note that mathematics is the study of uninterpreted formal systems. These

systems are themselves structures. Recalling what we said in the previous section, this means that they are systems of relations. The structure itself can be named (e.g. "group", "ring", "category", "algebra"), and it can be intrinsically described by indicating the relations among its relata. These relations and their relata constitute the structure. Now recall that the term "structure" is ambiguous, in that it can be used to refer either to a particular, or to a structural property. It is important to realize that the structures with which mathematics are concerned are always kinds of structures, viz. structural properties. Thus a term like "group" does not name a particular. Rather, it names a structural property.

In mathematics, the relata of these structures are themselves nothing but terms. That is, they are names without referents. Thus arbitrarily chosen letters are usually used to designate the relata. In applying the mathematical structure to some specific use, the relata are given contents: that is, the letters will genuinely refer. Now, it needs to be pointed out that I am using "content" here in a more general sense than it was used in the last section. Here content is used to refer to any intrinsic property or intrinsic property token. Thus, depending on the use to which the mathematical structure is put, its relata may be either structural or non-structural properties, or tokens of these properties. Perhaps I should also state here that what a particular is is a property-token, or a

cluster of property-tokens.

I will now provide an example to help provide some substance to these claims. A very important type of structure in mathematics is that of the group. In describing the structure of a group, some special symbols are used. But this does not alter the fact that the symbols are uninterpreted, and hence have no content. For instance, in any group there is an identity element, designated by e, satisfying the condition: $a.e = e.a = a$, $a \in G$: But the fact that "e" designates a special relatum in the group does not prevent "e", when interpreted, from being almost anything. The only restriction on the interpretation of "e" is that whatever provides its content must satisfy the condition stated above.

There are also special relations of the structure which are referred to as "operations". For instance, in the group structure, there is the product operation (.). It operates on any two relata of the group structure to produce another relatum of the structure. These relations are also uninterpreted, in that the product could be interpreted as any of a large number of specific operations on relata, depending on how the relata are interpreted. The fact that even the relations of mathematical structures are uninterpreted indicates the level of abstraction on which mathematics operates.

That a given structure is referred to as a group indicates that it has certain types of internal relations.

In the case of groups, these relations are:

(1) The product of any two relata is a relatum of the group--i.e. the structure is closed.

(2) (a.(b.c)) is the same relatum as is ((a.b).c).

(3) There is a relatum e such that for any relatum, a, (a.e), (e.a), and a are all the same relatum.

(4) For every relatum a, there is a relatum a' such that (a'.a), (a.a'), and e are the same relatum.

Admittedly, the terminology I have used here is a bit strange, but there is purpose to this madness: I wish to make it clear that such structures fit into the general ontological principles which I introduced in the previous sections.[16]

Note, in particular, that what counts as a group is defined by giving a list of relations which hold between the relata of the group. What counts as the unit element (e) is defined by its relations to all the other elements. And what counts as an inverse of any relatum is defined by its relations to other elements and the unit element. Thus, we see that the group is given an intrinsic description, and the unit element and inverses are described extrinsically. Prior to some specific interpretation, neither the unit element nor the inverses, nor any relatum of the group, can be provided with any intrinsic description. The group itself could be extrinsically described by giving its relations to other mathematical structures.

Physics

The structures which occur in physics are interpreted

mathematical structures. Thus physics provides some content for some mathematical structures. Now, providing content for mathematical structures is not physics' goal. Rather, it seeks to describe the fundamental physical processes of reality. It just turns out that, in performing that task, physics provides content for many abstract mathematical structures.

As an example of mathematical structures, we briefly discussed groups. To carry on with that example, we can look at interpreted group structures in physics. Examples of such are the Galilean group and the Lorenz group. These are examples of groups of coordinate transformations. That is, the relata of these groups are coordinate transformations. These transformations obey the rules listed above which define the group structure. In this case, the product operation of the group structure is interpreted as iteration of transformations. That these interpretations are genuinely such, and not merely the tacking on of fancy names to the relata, is shown by the fact that the interpreted relata (the transformations) are themselves intrinsically describable. That is, Lorenz transformations have a different intrinsic description from Galilean transformations.

To fill out this example, we will look at the Lorenz group. The relata of this group, the Lorenz transformations, are characterized by Einstein as being any equation such that:

it expresses x', y', z', t' in terms of linear

homogeneous functions of x, y, z, t of such a kind that the relation:

(1) $(x')^2 + (y')^2 + (z')^2 - c^2(t')^2 = x^2 + y^2 + z^2 - c^2t^2$

is satisfied identically.[17]

Now it should be noted that the above description is an extrinsic description of the Lorenz transformations, as it indicates their relation to another equation (equation (1)). This extrinsic description is independent of the extrinsic description of the Lorenz transformations as forming a group.

But as I have noted, Lorenz transformations can be described intrinsically. In order to do so, one must pick a specific such equation and describe it. Here is an example of a Lorenz transformation:

(2) $$x' = \frac{x - vt}{\sqrt{1 - v^2/c^2}}$$

This equation is the Lorenz transformation between two reference frames which are moving relative to one another only in the x-direction. (i.e. along the x axis). And let me remind the reader that it is the members of the set of all equations (of which (2) is an example) which satisfy (1) which are the relata of the Lorenz group.

To see how it is possible to provide intrinsic descriptions of equations such as (2), it is important to realize that they are, themselves, structures. That they are structures is what makes intrinsic descriptions of them possible. Clearly, equation (2) states a complex relation between x' on the one hand, and x, t, and the constant c,

on the other hand (recall that v is dx/dt). Thus the equation constitutes a structure whose relata are x', x, t, and c, with relations like: "x' is directly proportional to x - vt", etc.

Thus we have an example here which clearly shows that when one interprets an abstract structure, such as the mathematical group structure, and hence provides it with content, that content may itself be structural in character. In this case, the relata of the abstract mathematical structure are interpreted as microstructures which are themselves coordinate transformations. The relata of these microstructures are then coordinate axes (x, t). The transformations tell us how the axes of difference reference frames are related.

Note now that coordinate axes are the means by which we metrize a space, or a space-time. They are means of giving <u>positions</u> relative to some arbitrarily chosen point (the origin). They give positions relative to the origin by providing distance measures in some arbitrary unit. So basically, coordinate axes are positioning elements. This leads us to ask just what positions are.

First of all, we might ask whether the position of a point of space-time could be regarded as an intrinsic property of that point. The answer to this question is a clear <u>no</u>. And the basis for this answer is our principle (P4). Consider: If a point is regarded as constituting a universe, then it clearly can have no position, just as it

is clear that our universe cannot be meaningfully said to have a position. Thus, positions are relational properties. A question as to the position of our universe would be meaningful only in a context where we discovered that what we had called "the universe" turned out to be part of a larger universe.

That positions are relations is a point that holds for absolutist theories of space-time as well as for relational theories of space-time. It must be emphasized that the difference between these two theories is a difference between what is allowed into the ontology to serve as relata for the position relations. That is, the issue between these competing theories is not over a claim that positions are intrinsic rather than relational properties. Rather, the issue which divides them is whether there exists an extra set of entities besides the material objects--viz. the points of space-time.

In an absolutist space-time, the extension of the space-time provides the basis for all determinations of positions. As I have noted, positions are relations. But what sort of property is extension? Clearly, it is an intrinsic property of space-time, since any region of space-time has its own extension, and could be meaningfully said to have that extension even if that region were the universe. Indeed, the universe itself does have spatiotemporal extension, and there is, by definition, nothing else for it to form a relation with. So extension is an intrinsic property.

But is it structural or non-structural? It seems that we must say that it is structural, for given any extension we can arbitrarily select regions within it and indicate the relations between those regions. However, what is peculiar about these structures is the rather arbitrary nature of their relata, and the fact that the relata are always, again, structures.

Thus it seems that space-time, if it is continuous, provides us with an example of an infinite hierarchy of structures. We may note here that the difficulty I raised with regard to such a metaphysics does not apply here, since that difficulty involved the claim that such a metaphysic could not distinguish between matter and empty space, and here we are dealing only with empty space.

Before leaving the topic of extension, let me note that the concept of spatio-temporal position ought to be distinguished from that of space-time points. The former, as I noted earlier, is a relational property, even in an absolutist space-time. But the latter, in any absolutist scheme, is something quite different. It may be treated as a primitive particular, or (and this is a view I believe to be more defensible) it may be treated as an infinite set of nested regions, or it may be treated as the limit of such a set.[18] In the last interpretation, the points would be the relata at the limit of the infinite hierarchy of extension structures. In any case, the points are not relations, unless the relationalist doctrine of space-time is correct.

According to that doctrine, space-time points are nothing more than relations among material objects.

Physics is concerned with more than space-time structures (let us ignore the claims of geometrodynamics). That is, not only does it deal with space-time and its structures (including such entities as the Lorenz group), but it also deals with masses, charges, various field-potentials, etc. So now we need to ask: What sorts of properties are these? Are they relational or intrinsic, structural or non-structural? To find answers to these questions, we must look briefly at the roles such variables play in physics.

We will first consider the property of inertial mass. I wish to argue that this is, first of all, an intrinsic property, and secondly, is a non-structural intrinsic property. That inertial mass is an intrinsic property is shown by the fact that it is meaningful to ascribe an inertial mass to the universe as a whole. In fact, we could even calculate the value of the inertial mass of the universe. This could be done simply by summing the masses of all the constituent bodies. Similarly, it is meaningful to ascribe inertial mass to any particle taken by itself, and regarded as a universe itself. That inertial mass is a non-structural intrinsic property is shown simply by the fact that in physics it is not analyzed into any more basic properties. In fact, mass is one of the basic variables of physics, and more complex properties may be defined in terms of it. Now it must be admitted that in claiming that inertial mass is

a non-structural intrinsic property, I am limiting myself to physics as it is today. That is, it may be objected that just because physics treats mass as a non-structural intrinsic property, that does not mean that it actually is such a property. After all, we may yet discover that mass is analyzable into a set of relations among yet more basic properties, so that mass would turn out to be a structural property. This objection is well-taken, but it is beside the point I wish to make here. All I am concerned to argue here is that as far as current physics is concerned, inertial mass is a non-structural intrinsic property.

Let us now look at how inertial mass is described in physics, and see what lessons are to be taken from such descriptions. Inertial mass may be described as a particle's propensity to resist acceleration. It is, of course, forces which produce accelerations. Thus, when regarded metrically, the inertial mass of a particle is a measure of that particle's response to a given force. The scale of this metric will be set up in such a way that it expresses the proportionate responses of different particles. For instance, all things being equal, in response to a given force, if particle A accelerates twice as fast as does particle B, then particle A has half the inertial mass of particle B. This relationship is expressed in Newton's second law:

$$F = m_i a \quad \underline{\text{or}} \quad m_i = F/a$$

To prevent a point of possible confusion, I should point out that this last claim concerning inertial mass is

not altered by the fact that all particles accelerate at the same rate in a gravitational field. This equivalence of acceleration is due to the fact (an accidental feature, so far as classical mechanics is concerned) that gravitational mass has the same value as inertial mass. Gravitational mass is the property of particles which determines the magnitude of the gravitational force they generate. (Hence at least one author has suggested that a less misleading term for gravitational mass would be "gravitational charge").[19] In a gravitational field, the force on a particle is directly proportional to the product of the gravitational masses involved in the interaction. Thus a particle with twice the gravitational mass is attracted to the earth with twice the force, but since its inertial mass is equal to its gravitational mass, it resists the force twice as much. So the two effects cancel one another, and all particles accelerate at the same rate when in the same gravitational field.

Inertial mass also enters into computation of variables such as momentum (both linear and angular) and kinetic energy. The appropriate relationships are these:

$$p = m_i v \qquad K.E. = \tfrac{1}{2} m_i v^2$$

These relationships allow for other means of determining the quantity of inertial mass of a particle. For instance, if a particle is brought to rest from a known velocity, and in so doing releases a known quantity of energy, its mass can then be calculated.

It may have been noticed that the last three equations

referred to could each be restated in such a way as to appear to define inertial mass in terms of other variables. That is, we have the equations:

$$m_i = F/a \qquad m_i = p/v \qquad m_i = 2(K.E.)/v^2$$

Doesn't this refute my claim that inertial mass is a nonstructural intrinsic property? That is, doesn't it show that mass is really a structural property, or even a set of structural properties, with the structures indicated by these equations? The answer is an unqualified no. First of all, each of these equations must be regarded as extrinsic descriptions of mass rather than as intrinsic descriptions of it. Surely an acceleration (say) could not be regarded as part of the microstructure of mass. Secondly, even if it were the case that these could be somehow regarded as intrinsic descriptions of mass, this would not vitiate the general claim I wish to make that physics makes full use of non-structural intrinsic properties. For if momentum (say) is somehow regarded as a relatum of the structure that is mass, then momentum would have to be treated as a non-structural intrinsic property. One could not then turn around and claim that mass is a relatum of the structure that is momentum.

I will assume that an adequate case has been made for the claim that inertial mass is a non-structural intrinsic property. But now we come to a crucial point, particularly crucial with regard to implications for later parts of this thesis. The non-structural intrinsic properties of physics, such as inertial mass, are known, in physics, only

by extrinsic descriptions. Not only are these properties not intrinsically describable (as is the case with all non-structural intrinsic properties), but we also have no acquaintance with them, or no knowledge of any other type, concerning them, so far as physics is concerned. Another way of putting this is to state that such properties of physics are known only by their effects, which is to say that they are known only by the dynamical relations they enter into. To see this, note that the descriptions I have given of inertial mass all refer to effects this property has in determining the interactions of one particle with other physical entities. For instance, we can describe inertial mass as the property which is responsible for a particle's resistance to acceleration, whether that acceleration be caused by the influence of a field or by a collision with a particle. Such descriptions are clearly extrinsic.

This fact has led some physicists and philosophers, especially those of positivistic bent, to argue that the attribution of inertial mass to individual particles, as an intrinsic property of them, is meaningless, or at least erroneous.[20] Rather, they claim, it is only meaningful as an expression of the modes of interaction of interacting particles. To do this is, it seems to me, to abandon any attempt at explaining the interactions of particles, and to do so for wrong-headed reasons. The only reason for making such a move that I know of would be a commitment to phenomenalist principles. But I believe that I showed in Chapter 2 that there are good reasons for adopting realist rather than phenomenalist principles. Hence I do not hesitate to regard

properties such as inertial mass to be genuine intrinsic properties just because we know nothing of their intrinsic character (again, so far as physics goes). In the claim that we have such limited knowledge, I am in agreement with the positivists. But since I do not define, or even tend to define, existence in terms of knowledge, I do not conclude that the attribution of such properties is meaningless. Thus I am asserting that properties such as inertial mass are real, non-structural intrinsic properties, but that we can describe such properties only by their effects, and indeed know them only by their effects (I am still restricting myself to the context of physics). This is to say that they are describable only extrinsically, and that we have no acquaintance with their character.

In being committed to the reality of such properties, I am willing to accept the implication that it is conceivable that we could be acquainted with them. Indeed, the key argument of Chapter 7 will be that we are, in fact, directly acquainted with some of them. The point I am trying to make here is that so far as physics is concerned, we have no direct acquaintance with them.

Let me now indicate how what was said concerning the three types of structural metaphysics applies to what we have developed here. As should be clear by now, I am claiming that physics exemplifies a form-and-content metaphysics, with properties such as inertial mass, gravitational mass, electrical charge, various field potentials, spin, etc.

filling the role of the non-structural intrinsic properties.

The alternatives to this interpretation of modern physics are the bare particular metaphysics, and the infinite hierarchy of forms metaphysics. The former metaphysic is represented by the positivist position recently discussed. That is, the positivists limit physics to descriptions of the interactions entered into by objects, and will not posit any "occult qualities" to explain those interactions. Thus, the basic particles which partake in these interactions must be regarded as bare particulars, as they are admitted to have only spatial relations for properties. Needless to say, the difficulties I raised with regard to the bare particular metaphysics apply also to this sort of interpretation of physics.

The interpretations of physics provided by anyone who holds the infinite hierarchy of structures metaphysic would allow that there are intrinsic properties of physical objects, such as mass, charge, spin, etc. But they would claim that these properties are themselves structures, and that structural analysis must be pushed to still greater depths. Now I readily admit that whether such properties are structural or non-structural is an issue which cannot be settled a priori. And I also admit that to assume that such properties may be structural has certain methodological advantages (e.g. it leads to research programs to try to find the deeper structures). However, I do claim that at some level we must

find non-structural properties, and at least as far as physics is concerned now, we have reached that level. Recall that I have argued that if there were not some non-structural intrinsic properties, then matter would not differ from empty space. I am claiming that it is non-structural intrinsic properties such as mass, charge, spin, etc. which serve to distinguish matter from empty space.

Before leaving our discussion of physics, one more issue needs to be discussed. The issue is this: Since I have been claiming that physics tells us nothing of the intrinsic nature of the non-structural intrinsic properties, am I not committed to the view that physics, and indeed all of science, is, in a profound sense, incomplete? My answer is: Yes and no. In other words, the issue is complex, and requires some discussion. First of all, if we regard the purpose of science to be one of providing an account of reality, and if an "account" is the same as a description, then science is not profoundly incomplete. It is only incomplete in the ways everybody knows it is incomplete: e.g. we don't have adequate theories of social systems, we haven't found the engram, we haven't yet unified general relativity and quantum mechanics, etc. These limitations are simply ones of not yet having descriptions we can hope to have. But to point out that we cannot describe the non-structural intrinsic properties is to point out a limitation on all description as such. That is, in that realm, science does not lack any descriptions, simply because there are no

descriptions to be had by any means. Thus this limitation on science is a necessary limitation on all descriptions.

The "yes" part of my answer to the question is to point out that there is more that can be known of the world than can be known by description. That is, there is such a thing as knowledge by acquaintance. This is nothing mystical; it is just the sort of knowledge we have of tastes, odours, colours, etc., provided we are not blind to any of these senses. And the limitation of physics is that it does not acquaint us with properties such as mass, charge, etc. But let me note once more that this does not mean that we are, in fact, not acquainted with any of these qualities. After all, the secondary qualities have not yet found their place in the scientific account of reality. Tying together these points is the business of chapter 8 of this thesis.

Chapter 4

THE NEED FOR PERCEPTS

Up to this point we have dealt primarily with metaphysical topics. The ventures into epistemology made so far have been an attack on foundational epistemology, and a discussion of description. I have argued that there are no descriptive claims which could serve as an indubitable basis upon which to construct an edifice of knowledge. Also, I have distinguished between intrinsic descriptions, extrinsic descriptions, and exemplification. Now we need to look at other aspects of epistemology.

Since we will be looking for a solution to the problem of secondary qualities, it is clear that we will require a theory of perception. After all, it is in the context of perception that the problem of secondary qualities arises. The theory of perception I will defend here is a type of representationist theory. That is, I will defend a view which claims that the content of perception is distinct from the external causes of perception. These contents of perception will be referred to as "percepts". These percepts will, as percepts normally do, carry a good deal of weight in the theory of knowledge. They will be the states of which we are immediately aware in perception, and hence will serve as keys to that of which we are mediately aware. Correlative with the notion of immediate awareness is that of acquaintance. That is, many of the characteristics of the percepts are ones with which we are

acquainted, but cannot describe. Thus the distinction between acquaintance and description, which I introduced in the last section, corresponds in some degree with the distinction between immediate and mediate awareness.

By this time the reader might wonder how I can adopt doctrines such as these, and yet claim to reject foundational epistemologies. In particular, if I claim that we have immediate awareness of percepts, and acquaintance with the contents of percepts, does that not imply that percepts, to some extent, carry their own interpretations with them (to speak metaphorically), allowing no room for error on our part? My answer to this last question is, with important qualifications, yes. How these claims do not lead me to adopt a foundational epistemology is an issue with which I will deal carefully. But I am getting ahead of myself. Before I discuss this issue, I need to defend the claim that the contents of perception are distinct from the external physical world. That is, I need to establish that there are such things as percepts.

POSITIVE ARGUMENT

My positive argument for percepts is certainly not new. It is, essentially, the argument from illusion. That is, I argue that the contents of perception are not external physical objects on the basis that the properties of the contents of perception differ from the properties of the external objects. Hence the contents of perception cannot

be the external objects. Examples that can be used to demonstrate this are extremely numerous, and familiar to everybody. But I will mention a couple of examples anyway. Both examples deal with visual perception.

First example: Although the contents of visual perception have depth, they do not have the depth of the external world. The depth of the visual percept seems to be related to the depth of the external world by something like a logarithmic function. A nice representation of such a function is presented by the focusing scale on a camera. Here is an example of such a scale:

2 2.5 3 3.5 4 5 6 10 15 30 ∞

On this type of scale, the same distance represents the difference between 2 and 2.5 feet as represents the difference between about 12 feet and infinity. In other words, if this scale accurately represented our percept, then the depth of our visual percept is such that just as much depth in it represents a half foot depth in the external world as represents an infinite depth. Now I do not wish to claim that the depth of the visual percept has <u>exactly</u> the above relation to external depth, but I do wish to claim that it has some such relationship. Also, the exact character of the relationship is amenable to experimental study: one need only have subjects estimate the distances of objects of unknown sizes and unfamiliar shapes (precautions are necessary to eliminate inferential clues in depth discrimination) and graph the function from such data. But some cases are

clear prior to any such study, and it is these which serve as evidence that the claim being made here is true. For instance, the perceived depth between the stars and the fuzzy object we call the Andromeda nebula is virtually nil; but the real depth between them is the difference between, say, 50 light-years on the one hand, and two million light-years on the other.

Second example: Look at an open book through a mirror. The content of perception includes an open book behind the mirror, with reversed printing on it. But in the external physical world there is only, say, a brick wall behind the mirror, and the only book around has proper printing in it. Thus the content of perception, and the nature of the external world, differ markedly.

Obviously, examples could be endlessly enumerated. But these cases are, I believe, sufficient to make the initial point that the properties of the perceptual content differ from the properties of the external world. Hence, I conclude, the content of perception is not the external world. Rather, it is an entity in our brains, which may be referred to as a percept.

Before I continue with a defense of the doctrine of percepts, there is an important point about these examples that deserves mention at this time: Notice that these examples, when used to argue for the existence of percepts, assume that we have knowledge of the character of the external world. The first example assumes, for instance, that we

know that the Andromeda galaxy is two million light-years away, and that the stars are much closer. The second example assumes that we know that there is no book behind the mirror, and that there is no book with reversed print, either. To point out that such assumptions have been made in arguing for the existence of percepts points out that my claim that there are percepts is a high-level physical theory. It is a claim which rests on other physical theories, in that it uses such theories as evidence in its argument. This point will be of great importance in clarifying how the doctrine of percepts does not lead one into a foundational epistemology, and will thus be discussed more fully in the next chapter.

REPLIES TO OBJECTIONS

I am not so naive as to think that the citing of two examples will serve to establish a position as controversial as representationism. Thus I will consider how other theorists might object to, or have objected to, this position. I will first consider a selection of objections which might be raised by theorists of various persuasions. Then I will consider the actual objections raised by another physicalist, D. M. Armstrong.

Miscellaneous Objections

The following paragraph represents (fairly, I hope) how many direct realists might respond to the argument I have just given for the belief in percepts:

You have failed to show that the content of perception differs from the external objects, simply because you have appealed to improper descriptions of our perceptual content. In the second example, you say that you see a book with reversed lettering behind the mirror. But this is not a proper description of what you see. What you see is a book in front of the mirror. Whenever you describe what you see, you are projecting, loading in expectations. Whenever you give a description that makes the wrong projections and produces the wrong expectations, you have provided improper, or false, descriptions. All illusions are cases of improper descriptions, false expectations, incorrect projections. Indeed, rather than not looking as it should look, the book looks just as it should look, under those circumstances. If you think you see a book behind the mirror with reversed lettering, then you have not yet learned to properly describe what you see. When you have adequate knowledge of mirrors, it will be perfectly clear to you that what you see is a properly-lettered book in front of the mirror.

I wish to argue that, although this type of move is perfectly compatible with an immaterialism such as Berkeley's, it is not compatible with the physicalism professed by modern direct realists.[21] That is, whether one adopts phenomenal entities, or physical objects, as the basic entities of the metaphysics, plays a very important role in determining the adequacy of the stated defense of direct realism.

Consider again the example of the mirror. In

Berkeley's system, there is a phenomenal entity which may be described as containing reversed letters. And when one says: "I seem to see a book behind the mirror with reversed letters", one is making no mistake. Any mistake occurs in one's expectations concerning the character of further experiences.

But the physicalist direct realist must say very different things about this example. He must say that there is no entity of any sort which has reversed letters, since the only perceived entities are the external objects, and the only book among the external objects has lettering which is right-way round. How then is he to explain the tendency we all have to say that we see an image of a book with reversed lettering behind the mirror? How does he explain our natural tendency to be misled in such situations? Berkeley, and other immaterialists, can easily answer such questions by pointing out that we have a percept which has reversed lettering, etc. in it. The physicalist direct realist can allow no such entity.

Let me argue now by appeals to my own experience. I am one who is quite familiar with mirrors. I know the basic principles which govern their influences on light, etc. I have also had plenty of experiences with mirrors in perceptual situations. Nonetheless, when I am put in the situation of the second example, I see an image of a book, with reversed letters, behind the mirror. I know that there is no book with reversed letters behind the mirror, but I see it nonetheless. I find that I cannot doubt that the content of my

perception includes such an image. Indeed; I even have great difficulty reading such a book, precisely because its letters are reversed.

If the direct realist is stubborn, he can make this sort of reply to the above remarks: You should remember that the image you see is exactly what is expected, given the physical laws and the arrangement of physical objects. You do not see a book with reversed letters. Rather, you see a book, with proper lettering, through a mirror. And what you see is just what you should see, given the laws of physics.

But there is another example with respect to which this last move is more difficult to make. The example I have in mind is one in which the laws of physics do not lead one to expect to see what one does, in fact, see. This example utilizes a plaster bust of a man's head, and a plaster cast of that bust's face.[22] The cast is an outside-in version of the bust. If one then places the bust and the cast side by side, both facing the subject, and the subject is, say, ten feet away, then the subject sees two busts. That is, neither the bust nor the cast appears to be turned outside-in. The only difference between the two is that they appear to be lighted differently. If the lighting is from above, then the nostrils of the bust are dark, while the nostrils of the cast are light. Even if the subject is told that one is a cast, and is told which one is the cast, and is made to realize that the apparent difference in lighting is due to this fact, the subject still sees what looks

to be two busts. This is an illusion that is not altered by such knowledge. If then the subject slowly moves back and forth (laterally) in front of them, the cast appears to gyrate back and forth in a rather spectacular fashion, while always appearing to be another bust, peculiarly lighted.

The relevant difference between this case, and that of the mirror, for our purposes here, is the fact that in this case the laws of physics do not lead one to expect this effect. They only lead to the expectation that the pattern of light and dark on the faces will be reversed. There is no reason, from the standpoint of physical optics, why one should expect the subject to only see two faces, both right side out. This means that the direct realist cannot claim that the image is what one would expect, given the laws of optics.

But suppose now that the direct realist claims that this image is what one would expect given physiological and psychological laws. The intent of this response is that there is no reason why we should limit ourselves to the consideration of physical optics only, and that when all the factors are considered, we see just what one would expect us to see.

At this point we need to step back and look at what is going on in this line of reasoning. By responding in this way, the direct realist has admitted what I take to be the key claim of representationism: viz. that the content of our experience differs in its properties from the properties

of the external objects of perception. We should also note that any representationism developed in the context of physicalism must agree that the differences are just what we should expect, given the laws of physics and physiology. So it is no longer clear just how it is that the line of reasoning we have been following could possibly constitute a defense of the direct realist position. Hence I shall consider this sort of defense to be unworkable. However, there will be more discussion of points directly relevant to this problem when I examine C. A. Hooker's version of direct realism in Chapter 7.

Another sort of objection that has been offered against representationism goes something like this: The argument that we do not really perceive external physical objects is essentially similar to an argument showing that when we call someone over the phone, we really don't talk to that person, but instead, talk to the diaphragm on the telephone receiver.

This argument aims to show that just because there is a causal sequence between oneself and the person on the other end of the line, this does not mean that one is not conversing with that person. Similarly, the argument goes, just because there is a causal sequence between our awareness in perception and the object which causes our perception, it does not follow that one is not perceiving the external cause of the perception.

There is an important element of truth in this argu-

ment. However, that element of truth does not harm the case for representationism. The representationist's claim is that the content of perception--i.e. the set of properties which makes up our perceptual experience--is located within the brain. The analogy in the phone example is the claim that the sounds we hear are those of the speaker diaphragm, and not the sounds of the other person's voice-box.

Now the element of truth which is contained in this argument is that the physical objects play a very special role in the perceptual relationship, and in some sense of "object of perception", it seems to make the most sense to speak of the external physical objects as being the objects of perception. To try to capture that element of truth, I will refer to the external objects of perception as the "intentional objects of perception". In this way, we capture the idea that the contents of perception--the properties of the percepts--are about the external physical objects. That percepts have this intentionality (whereas, say, a photograph does not, at least not in the same sense) is due to the fact that they play important roles in determining the actions of an organism. Many of these actions are directed toward the external objects, and it is the nature of the percepts which guide these actions. This, of course, raises the whole nest of issues of the analysis of "purpose", "intentionality", and like notions. I will deal with these notions a bit more at a later point.[23]

In any case, it should now be clear how we can deal

with the phone example: It is, indeed, absurd to claim that we are not talking to our Aunt Bessie, but are really talking to the phone diaphragm. Likewise, it is absurd to claim that we do not look at tables and chairs, but really look at our own percepts. What is absurd about such claims is that "talking to" and "looking at" are intentional relationships, and the intentional objects of such relationships are Aunt Bessie in the phone example, and external tables and chairs in the perceptual case. But none of this alters the fact that the perceptual content is internal, just as it does not alter the fact that, in the phone example, the qualitative character of the sounds we hear are those of the phone diaphragm (assuming, for the sake of this example, naive realism).

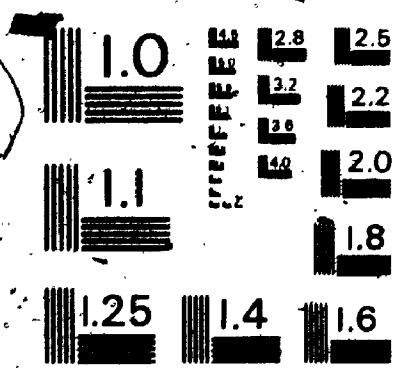
I must emphasize quite strongly, that although I advocate a representationist theory of perception, I am a physicalist. Thus I claim that the percept is a physical entity. More specifically, I claim that the percept is a state of some part of the brain. By making such a claim, I am immediately confronted with the type of argument displayed in the next paragraph.

The properties of our percepts cannot be properties of some part of the brain, simply because neurosurgeons, upon laying bare the brain of a conscious patient, never see, anywhere on or in the brain, the characteristics of the visual experiences the patient is presently having. If, for instance, the patient is seeing a green patch, the surgeon never sees

a green patch in the patient's brain. Surely, if one claims that the contents of perception are brain-states, then one should expect the surgeon to see the same things, in the patient's brain, that the patient reports seeing. But all the surgeon ever reports seeing is a gray spongy material, or a white fibrous material, which has certain electrical and chemical properties.

This argument presents no real problem for my position, since it rests on a confusion between theories of perception. In particular, it applies a representationist theory to the patient, but applies a direct realist theory to the surgeon. It applies the representationist theory to the patient by expecting him to have the properties of his percept in his brain. But it then applies a direct realist theory to the surgeon by expecting him to directly perceive the properties of the patient's brain.

There is only one sort of theory which would lead us to expect the surgeon to see what the patient sees. That would be a theory which claimed that the causes of green percepts were themselves green. But there is no a priori reason to expect that this should be so, and the facts appealed to in the objector's argument provide empirical reason to believe that it is not so. Thus we must say that the causes of colours are not themselves colours, and will probably also have to say that the causes of tastes are not themselves tastes, etc. But now we are encroaching on the problem of the secondary qualities, and now is not the time to discuss



that problem.

But aside from the secondary qualities: What is the representationist to say about what the neurosurgeon sees with respect to the primary qualities? Surely we do not wish to say that the primary qualities of experience, such as shapes, are not caused by similar properties. I do, in fact, wish to claim that the primary qualities of percepts are caused by essentially the same qualities in external objects. Why then, it may be asked, does the surgeon not see these properties? The answer here is that, with special procedures, he can see such properties of the percept in the patient's brain. But at this time I will rest content with this bold statement, and save the elaboration of this example for the last chapter of this thesis, where I shall discuss more generally the physiological evidence which is relevant to claims made in this thesis.

But one brief poiht can be made here. Once one realizes that there is no reason to expect to see the colours of a patient's visual percept in his brain, one can no longer expect to see the shapes seen by the patient in his brain. After all, we always pick out shapes in our visual percepts by means of the colour medium. That is, we see shapes because of the colour varigations in our visual field. So since we cannot see the colours of the patient's percept, we can hardly expect to see the shapes of his percept.

The last of the arguments I will consider in this section has been put to me like this: Either representation-

ism is contingently true or necessarily true. If it is the latter, then it is logically impossible to perceive the external world and hence Occam's razor can be applied to eliminate the external world. Thus representationism would collapse into idealism. On the other hand, if representationism is contingently true, then we can properly apply scientific evidence to decide its truth. And there is plenty of such evidence to show that we do perceive the external world rather than our own brains.[24]

First of all, for reasons that will become clearer when I discuss semantics a bit more (Chapter 6), I do not place much stock in the contingent-necessary distinction. However, if we understand by "contingently true" the idea "true as a matter of fact, and subject to empirical confirmation or disconfirmation", then my version of representationism must clearly be counted as contingent. After all, it is based on the claim that, as a matter of fact, the contents of our experience do not have exactly the same properties as does the external world, even when aspects of the latter are serving as intentional objects of perception. I believe the evidence backs up this position. That is, I dispute the claim that science provides evidence that we experience the external world, except in the sense that the external world provides the intentional objects of perception. All the evidence that indicates that some of the properties of the contents of perception differ from the properties of the external world serves as evidence for the

type of representationism I am defending here. And the evidence that does show that we do "perceive the external world" is actually evidence that shows that the intentional objects of perception are the external objects. And that the external objects are the intentional objects of perception is something, the reader will recall, that I do not dispute.

Armstrong's Objections

The objections I have considered so far could have been advanced by philosophers of quite varied persuasions. However, physicalists have been especially vociferous in their objections to representationism. And since such arguments are more likely to share some of my beliefs, I need to devote some space to answering such arguments. Here I shall limit myself to answering some arguments presented by D. M. Armstrong.

To begin with, Armstrong acknowledges that the problem of illusions provides incentive for adopting representationism:

> How is this perceptual experience to be conceived? Suppose I have the perceptions that we associate with looking at a red ball. It is clear that I might have had exactly the same perceptions without there being any red ball in physical reality. When we reflect on this point it is very tempting to say that what is involved is some relationship between my mind and a non-physical red item: a sense-impression or a sense-datum. Now it is clear that if there are such items involved in perception, then it is false that perception is simply a state of the person apt for the bringing about of certain physical behavior or a state of the person apt to be brought about by certain physical stimuli. The 'Causal' analysis of the concept of perception would be false.[25]

I am one who makes the move to adopt a sense-impression, but I hold it to be a physical entity. Why Armstrong is unwilling to make that move is due, I shall argue, to his construal of the problem of secondary qualities. I shall discuss that issue, and Armstrong's construal of it, in Chapter 7.

But at this point, I will consider some specific arguments presented by Armstrong against representationism. An argument on which he places much stress is "the paradox about the non-transitivity of the relation 'exact similarity in a given respect' with regard to the alleged sensory items." Armstrong presents the case very clearly, so I hope the reader will forgive me if I quote him extensively here:

> If A is exactly similar to B in respect X, and B is exactly similar to C in respect X, then it follows, of logical necessity, that A is exactly similar to C in respect X. 'Exact similarity in a particular respect' is necessarily a transitive relation. Now suppose that we have three samples of cloth, A, B, and C, which are exactly alike except that they differ very slightly in colour. Suppose further, however, that A and B are perceptually completely indistinguishable in respect of colour, and B and C are perceptually completely indistinguishable in respect of colour. Suppose, however, that A and C can be perceptually distinguished from each other in this respect.
>
> Now consider the situation if we hold a 'sensory item' view of perception. If the pieces of cloth A and B are perceptually indistinguishable in colour, it will seem to follow that the two sensory items A_1 and B_1 that we have when we look at the two pieces actually are identical in colour. For the sensory items are what are supposed to make a perception the perception it is, and here, by hypothesis, the perceptions are identical. In the same way B_1 and C_1 will be sensory items that are identical in colour. Yet, by hypothesis, sensory items A_1 and C_1 are not identical in colour![26]

This whole problem is based on the idea that we cannot dis-

tinguish continuously differing qualities. The qualities must have some minimal finite difference to be distinguishable. Now it seems to me that there are at least two different models of how we might react, as perceivers, to qualities with continuous differences:

Model (1.) Qualitative continua, such as that of colour, are actually split into distinct perceptual "quanta". Any two qualities (external causal qualities) affecting our sensory capacity within a single perceptual quantum, are then perceived as qualitatively identical--i.e. they produce identical percepts.

If this is the proper model of the perceptual process adopted by the representationist, then he must claim that the paradoxical situation as presented by Armstrong would be physically impossible. For if A and B are within a quantum, and B and C are within a quantum, then A and C must be within the same quantum, and hence the percepts they produce will be indistinguishable. If, on the other hand, A_1 and C_1 are distinguishable, then either A_1 and B_1, or B_1 and C_1, must be distinguishable. That is, either A and B, or B and C, must lie within different quanta. This leaves us with the empirical question of whether situations such as Armstrong depicts do actually occur.

Model (2.) Qualitative perception is always comparative, and the difference between two qualities must exceed a minimal value if they are to be distinguished. On this approach, the percept is claimed to preserve the minute

differences in colour of the stimuli.

If this is how the representationist depicts the perceptual process, then the case on which the "paradox" rests is physically possible. But is there a problem? The problem, as Armstrong sets it up, turns on the notion that if distinguishability is a matter of having percepts, then when the qualities are indistinguishable, the percepts must be identical. But to set up the problem in this way requires that model (1) be adopted, since it presumes that the receptors quantize the perception ("receptors" is here taken to include all processing of input up to the point of conscious awareness). And as we have seen, the paradoxical state of affairs is simply denied within model (1).

Thus we can see that Armstrong's argument makes two key assumptions: First of all, it assumes that the representationist must adopt model (1), and secondly, it assumes that the situation as depicted by Armstrong actually occurs. Now if we suppose that the second assumption is correct (and this could be empirically tested), then Armstrong's example serves to refute any representationist who adopts model (1). The shortcoming of Armstrong's argument is that it ignores the possible representationist model (2). Now we must consider more carefully whether or not Armstrong's argument poses any problem at all for model (2).

Model (2), again, depicts the percept as having preserved the qualitative continuity of the colours. The parts of the percept which represent the two colours must then

have a minimal colour difference before the subject will be aware of them as different. In short, model (2) places the distinguishing process in the awareness of a minimal difference in the percept. Now what occurs in this process is, indeed, problematic. But it is a problem for any theory of perception: the direct realist also has the problem of accounting for this type of awareness of that which is directly perceived. Thus it is the case that the only problem raised by Armstrong's example, within model (2), is one which occurs for any theory of perception. Hence the argument cannot serve as an argument against representationism as such.

Armstrong's next argument against representationism is an appeal to the indeterminacy of perceptions: We see a spotted hen, but we do not see a hen with, say, 543 spots. Thus, it seems, the percept must actually have an indeterminate number of spots. But surely it is the case that no actual entity can have an indeterminate number of spots. Such is the problem as Armstrong sets it up.[27]

I fail to see any real problem here, unless one wants to claim that our awareness of our percepts is "self-intimating"--i.e. unless one claims that we are necessarily aware of every aspect of a percept. Such is, indeed, a traditional claim of many representationists, but is not a claim I wish to make. And of course, I am claiming that the "self-intimating" thesis is not a necessary correlate of any representationism. I see no reason to make any such claim, any more than do I see any reason to claim that we are fully

aware of all contents of our mind, our brain, or any other part of our body. If the speckles of the hen are far enough apart (in relation to our distance from the hen) so that our receptors can resolve the spots, then our percept of the hen will have as many spots as does the hen (on her visible side). If the spots are too close together to be resolved, then the percept will have fewer spots than does the visible side of the hen. But suppose the spots are resolved, and the visible side of the hen has, say 543 spots, so that the percept also has 543 spots. We cannot see that there are 543 spots unless we have the chance to count them. The point here is that we do not have to be conceptually and consciously aware of all features of our percepts. Indeed, it is most likely the case that we are never totally conscious of an entire percept, especially a visual one. I might add at this point that the approach I took by adopting model (2) above also involved a denial of the thesis that our percepts are self-intimating. There it was claimed that we need not be aware of the minutest differences in the qualities of our percept.

Now Armstrong does speak to this reply. He says:

> The theory is now postulating (i) speckled physical surfaces with perfectly determinate characteristics; (ii) speckled sensory items with perfectly determinate characteristics; (iii) indeterminate awareness of the speckled sensory items. But have not items (ii) become redundant? Why not simply postulate the speckled physical surfaces and indeterminate awareness (perception) of those surfaces?[28]

The reason why we do not dispense with the percepts in this case is that other cases have led us to postulate their existence. They are needed to account for other data. And since

they are postulated as real physical entities, we cannot have them popping in and out of existence when it suits us. Consider this analogous situation: In astronomy, the fact that the heliocentric theory is extraneous in relation to navigational purposes does not serve as an argument against the heliocentric theory. It just leads us to recognize that the geocentric theory is a quite adequate hypothesis for some purposes. That it was adequate for many purposes helps explain why it was believed so long. Similarly, the representationist theory of perception is extraneous in relation to cases of indeterminate perception of the sort being considered here. But this fact should only lead us to recognize the fact that the naive realist theory of perception is a quite adequate hypothesis for some purposes. And this fact helps explain why it has been believed by so many people for such a long time.

There is one more objection to the representationist theory of perception raised by Armstrong which needs to be dealt with.

> In the representative theory, the Mind is confined to non-inferential knowledge of its own sensory items, and has to make an inference to the existence of physical things.[29]

If we allow a liberal enough interpretation of the term "inference", then this statement is true. But so what? How does this present a serious problem? Perhaps the bite which Armstrong, and others, see behind this fact can be expressed in the following way: This situation is serious because it puts us in the situation of being only able to guess as to

the nature of the causes of our perceptions. And if this is so, how are we ever to be assured of any certainty in our pursuit of knowledge?[30]

My answer is this: Whether one adopts the representationist theory, or whether one claims that we directly perceive ordinary physical objects, the same sort of problem arises: How are we ever to know for sure, or even with some finite probability, that our claims as to the nature of reality are true? In either theory of perception, we have direct awareness of some aspect of reality--either percepts or visible external objects. The problems arise in projecting any claims about the nature of unobserved reality. Such problems, of course, constitute the notorious problems of induction.

I wish to say some things about the problems of induction, and about the inductive process, and I will do so in the next chapter. However, at this time there is another point that can be made, which might help dispel the type of misgiving we have been discussing. Recall the arguments which I used as positive arguments for the representationist theory of perception. I noted, at that point, that the arguments assumed the correctness of our theories of physical reality. Without such assumptions, the arguments could not have gotten off the ground. Indeed, all arguments from illusion make this sort of assumption: they assume an account of physical reality, compare the content of our perceptions with this account, and find that they differ. This leads

to the postulation of the existence of percepts. To then claim, on the basis of the theory of percepts, that we know nothing of external reality, would be absurd. To reject our theories of physical objects is to reject all the evidence I have presented for believing in percepts. So if one goes, they both go. If one rejects the theories of external objects, then one is also obliged to reject the theory of percepts. So rather than making these absurd moves, the proper thing to do is to recognize that the theory of percepts is itself a physical theory of essentially equal status with other physical theories.

Chapter 5

ERROR AND CERTAINTY IN PERCEPTION

I have indicated before that in adopting a representationist theory of perception I am bringing with it the claim that we are immediately aware of, or are acquainted with, the characteristics of our percepts. And I have also indicated that such immediate awareness is the sort which allows no room for error. It will also be recalled that in Chapter 2 I argued that there are no claims which can serve as indubitable foundations of knowledge. I also claimed, in the last chapter, that our percepts are not self-intimating. In the context of these discussions, the question may have occurred to the reader: How are all these claims to be rendered compatible? In this chapter, I will develop these themes further in the context of a general theory of error. And hopefully, the end product of this endeavor will be seen to be quite coherent.

Given the number of interconnecting issues involved in this chapter, we need a plan of attack. That plan goes like this: First I will develop a general theory of error: That is, I will attempt to clarify just what sorts of entities are mistakes, again in a physicalist context. Then I will look again at the three claims I mentioned at the beginning of this section, viz. (1) immediate awareness of percepts

allows no room for error, (2), there are no claims for which we have the a priori certainty requisite for foundations of knowledge, and (3) percepts are not self-intimating. These will each be considered in the context of the general theory of error, with an eye to showing how all of these claims fit together into a coherent system of thought. Finally, brief accounts of explanation and induction, and a justification of the latter, will be presented in the context of the account of certainty and error developed in the previous sections. These developments are also needed to help fill out the account of our knowledge-system as non-foundational.

THEORY OF ERROR

To clarify what mistakes are, we need to concentrate on the idea that a mistake is a matter of having one's expectations differ from the actually occuring event. The development of the expectations can occur in accordance with the physical laws governing information processing in the brain, and the development of the externally occuring event can occur in accordance with physical laws, without them leading to the same result. This is possible simply because the laws governing these two sets of interactions are different sets of laws. The schematic diagram of Figure 2 might help clarify this point.

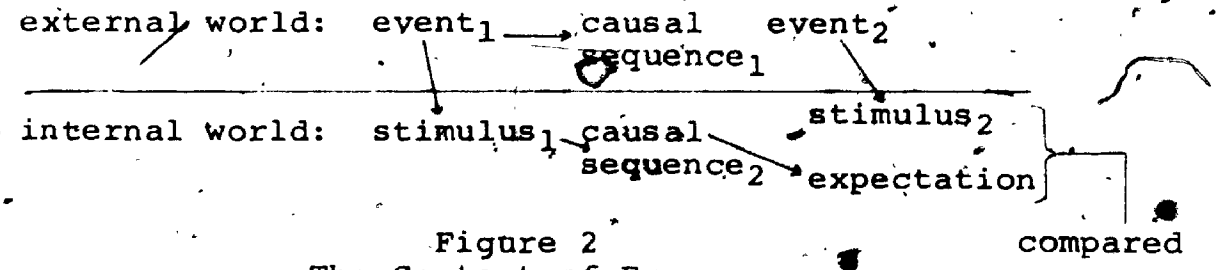


Figure 2
The Context of Error

In Figure 2, event$_1$ produces, in us, stimulus$_1$. Of course, the event which causes the stimulus is not the same thing as the stimulus. And, the causal sequences set off by each also differ in important respects. The causal sequence$_1$ may be a set of relatively simple physical interactions, while the causal sequence$_2$ may be a very complex set of neuronal interactions. This latter sequence is one of an information processor, while the former is not. For example, suppose event$_1$ is a rock flying toward a window. As a first approximation, stimulus$_1$ can then be regarded as the percept of the rock flying toward the window. The causal sequence$_1$ is then the rock covering the remaining distance to the window and striking it. The causal sequence$_2$ is then all the complex neuronal processes that are involved in producing an expectation that the rock will break the window. In some cases, this would include thinking about what is going on, with references to relevant theories and their relations: Event$_2$ is then the rock bouncing off the bullet-proof window pane, which causes the corresponding percept (stimulus$_2$) in the observer's brain. But stimulus$_2$ does not match the expectation. This is to say that the expectation is wrong, and that the observer was mistaken.

It is clear that if error is to be avoided, then the two causal sequences of the diagram must have some sort of structural similarity. Failure to have the appropriate similarities leads to expectations which diverge from the compared stimuli.

There are certain respects in which the account given so far, with reference to Figure 2, is oversimplified. In particular, clarification of the role played by the percept in this scheme is required. In providing this clarification, we will also be able to see just why it is that we cannot be mistaken in our acquaintance with the content of our percepts.

The discussion of the previous chapter proceeded on the assumption that (as a first approximation) the percept is that product of our perceptual system of which we are consciously aware. As a result of the discussion of that chapter, particularly the discussion of Armstrong's criticisms of representationism, I adopted the position that we must not require that the percept be self-intimating. The adoption of that position requires a refinement of the view that the percept is simply that product of the perceptual system of which we are consciously aware. In place of that simple view, I now propose that we regard percepts as being <u>those products of our perceptual systems which are directly accessible to conscious awareness</u>. Being <u>directly accessible</u> to conscious awareness is not the same as being a content of conscious awareness. That is, at a given time, the perceptual contents of which we are consciously aware do not exhaust the contents which are directly accessible to our consciousness at that time.

An example may help clarify what I am talking about here: In vision, we can look at a particular scene, hold our gaze steady, yet let our conscious awareness wander around

the visual field. We can be conscious of the detail of the object upon which we have fixated, or we can become conscious of the lack of detail in the peripheral regions of the visual field. The visual field, as referred to here, is taken to be identical with the visual percept: it is all directly accessible to our conscious awareness; but we are never, at a given moment, fully conscious of all of it.

A bit more needs to be said here concerning conscious awareness. In particular, more must be said concerning the notion of direct accessibility to consciousness. In Chapter 7 of this thesis, in my critique of C. A. Hooker's attempted solution to the problem of secondary qualities, I shall discuss two models of consciousness: one treats consciousness as a subject-object relation, while the other treats it simply as an intrinsic property of the internal state of the (in this case, perceptual) system. At that point, I will indicate some reasons for adopting the latter model. So here I will characterize "direct accessibility to consciousness" in terms of that model.

In the absence of a detailed theory of consciousness, let us refer to consciousness as a property that can be had by given regions of the brain. Also, a key claim of this thesis (to be developed further in Chapter 8) is that the visual percept (say) is a particular sort of state of an extended region of the brain. Now if we refer to the property which is consciousness as the C-property, we can express the idea that the visual percept is that part of the visual per-

ceptual system which is directly accessible to consciousness in this way: The percept is that region of the neural visual system which is capable of having the C-property. Thus when we shift our attention within our percept, according to this model the region of the visual cortex which has the C-property varies. As it were, the C-property flows from one region of the visual cortex to another.

In the context of this (admittedly crude) model of consciousness, and in the context of the theory of error sketched earlier, it becomes clear why we cannot be mistaken concerning the contents of our percepts: To be in error concerning an event, there must be sufficient room between the content of our consciousness and the event to allow for the type of structure depicted in Figure 2. That is, there must be the development of an expectation, and a comparison of that expectation with a stimulus. But in our consciousness of a percept, the consciousness is simply a property of the percept (according to the model introduced here). There is no sense in which the consciousness can develop an expectation and compare it with an input, all within the state which is the percept-with-C-property. Being in error is a function of the whole perceiving-and-acting system. It is not a function within the capacity of a state or part of that system: specifically the state which is the percept-with-C-property.

With the role of the percept and consciousness in the theory of error somewhat clarified, we can now discuss key oversimplifications implicit in Figure 2. One important point to note is this: If we are to regard stimulus$_1$ of Figure 2 as being the immediate results of physical interaction with the world outside our skin, then stimulus$_1$ cannot be equated with a percept. The percept is the result of a good deal of information-processing on the part of the organism. In the case of vision we can regard the processing to begin even at the cornea, where there is some absorption and reflection (i.e. selection) of the light. At the lens, encoding begins, with the light being focused on the retina. As is well-known, the retina has a very substantial encoding and selecting function, carried out by the complexes of retinal ganglion cells. This processing continues in the lateral geniculate nuclei, from which the information is passed along the optic radiations to the occipital cortex. In this thesis, I adopt the hypothesis that the location of the percept is in the striate cortex of the occipital lobe. I shall discuss the evidence for this hypothesis toward the end of the thesis.

In any case, once it is evident that stimulus$_1$ is not to be identified with the percept, it becomes clear that the responsibility for error need not rest with the causal sequence which lies between the percept and the production of an expectation. Error could arise even in the production of the percept. Now we must be quite careful here in our use of the term "error". Percepts, taken in isolation,

cannot be said to be mistaken. This is so because it is the production of the wrong expectation which constitutes a mistake, and it is the entire processing system, all the way from $stimulus_1$ to the expectation, which is involved in the production of the expectation. Presumably, many different alterations in the processing system might succeed in producing the proper expectation.

The percept itself, as I noted above, occurs somewhere along this line of information-processing, and is to be identified by its being the perceptual content which is accessible to consciousness. Since it is the expectation which may be said to be in error, then it is the system as a whole which produces that error which is the source of that mistake, and is to be held responsible for the error. When a mistake occurs, the error does not, so to speak, point to any particular part of the processing system as its source.

Now, there is a sense in which we could speak of a particular part of the system as being responsible for the error: A particular part of the system can be taken as the source (in an unequivocal manner) if, and only if, an alteration in that part, but in no other part, will produce the correct expectation. However, this sense of responsibility for error is, as a matter of fact, never applicable in systems as complex as the human nervous system: there are always several different changes which would produce a correct expectation in any given situation. This leaves us with the fact that the responsibility for error can be attri-

buted to particular parts of the system only in an equivocal manner. That is, a change in that part would produce a correct expectation, but other changes in other parts would also produce the correct expectation. Because of the equivocal manner of such attribution of responsibility for error to parts of the system, I claim it is only correct to attribute error to the expectation itself, and to attribute the responsibility for the error to the system as a whole which produced the expectation.

Let us consider an example in order to add some substance to these rather abstract claims. Consider the well-worn example of someone who sees a stick in a glass of water (as we all know, the stick appears bent), and expects it to feel bent when he grabs it. I am claiming that his error is constituted by his expectation that the stick will feel bent, and that the source of the error can be unequivocally regarded only as the system as a whole, from the stimulus to the expectation, which is involved in the production of that expectation. Now, consider these alternatives: Should we attribute the source of error to the part of the system which produced a bent-stick-percept, or should we attribute the source of error to the system between the percept and the expectation (this latter would include our consciously controlled beliefs)? I am here claiming that there is no unequivocal way to choose between these alternatives. Either sort of change would serve to produce the proper expectation in

this situation.

If we were to introduce a pragmatic element here, and treat the source of error as being that which is most easily altered, then we would locate the source of error in the processing system which occurs after the percept, for it is within this part of the system that there are aspects subject to our conscious control. These include our consciously-held theories and beliefs.[31]

It should be clear from what has been said so far that only the sort of entity which can have expectations can ever be said to be mistaken. Thus I would say that the reason why inanimate objects such as rocks can never be said to be mistaken is that they are not the sorts of entities which can develop expectations. If this distinction is to be of much use, more needs to be said about just what conditions are required for an organism to be capable of producing expectations. A more thorough exploration of this problem in terms of specific examples can be found in the Appendix to this thesis. However, we can summarize the main points briefly here.

For an organism to have expectations it must have these features: It must have an internally produced state (the expectation) which is compared with an externally produced state (the $stimulus_2$), and it must be capable of responding to a disparity between these states by altering the processes by which it produces the internally-produced state so as to bring future internally produced states in

line with future externally-produced states. This type of process is an example of what is referred to in cybernetics as a negative feedback loop. Another feature that is required is that the organism have an internal purpose. That is, its responses must be such as to be directed to its own survival and well-being, rather than directed to fulfilling some other being's purposes (the latter I refer to as external purposes). As explained in the Appendix, this last requirement allows for the distinction between mistakes and malfunctions.[32]

SYNTHESIS

Now that we have developed a general theory of error, we need to see how three of my claims, which may at first sight appear inconsistent, can be shown to be consistent. Those claims are: (1) immediate awareness of percepts allows no room for error, (2) there are no claims for which we have the a priori certainty requisite for foundations of knowledge, and (3) percepts are not self-intimating. I will consider each of these claims in turn.

1. We can summarize the points made in the last section concerning our immediate awareness of percepts, in the following way: In the physical process which is the acquaintance with the percept, there is no production of an expectation and resultant comparison with a new stimulus: There is just simply the acquaintance with a percept. And

without the production of an expectation, there can be no question of any error. Indeed, the acquaintance with a percept is required by the process of comparing an expectation with a percept.

The only sense in which we could ever say that error occured in our acquaintance with a percept is in the derivative sense in which a part of the process between the $stimulus_1$ and the expectation is singled out as the unique part which could be changed and produce the correct expectation. But we saw earlier that this attribution is always equivocal, and we also saw that pragmatic factors lead us to localize errors in that part of the process from the $stimulus_1$ to the expectation which is amenable to our control. Such pragmatic considerations also prevent us from attributing error to our awareness of a percept, because we cannot choose the kinds of our acquaintance with our percepts. We are acquainted with (say) two colours in a percept as being different in a particular kind of way, and we cannot choose to be acquainted with that percept any differently. For these reasons, I stand by the claim that there is no room for error in our acquaintance with percepts.

2. But if there is no room for error in our acquaintance with our percepts, why may we not appeal to this as a foundation for knowledge? Well, the reason we may not do so

is that we do not know a priori that our acquaintance with our percepts is incorrigible. Indeed, we do not even know a priori that we have percepts: My claim that there are percepts was, as I noted before, based on empirical claims, and involved appeals to rather high-level theories concerning the nature of the physical world. And my claim that our acquaintance with our percepts is incorrigible has been based on a theory of error, a theory of percepts, and a model of consciousness, none of which I would claim to be a priori indubitable truths. In short, the claim that there is no room for error in our acquaintance with our percepts is based on highly contentious claims which are not self-evident truths. So if any of those other claims should be found to be mistaken, we would perhaps be forced to abandon the claim that our acquaintance with percepts is incorrigible. So in arguing that our acquaintance with percepts is indubitable, I am already quite high up in the edifice of knowledge (supposing for the sake of argument that knowledge forms some sort of edifice), and such is no place to be pretending to lay a foundation for that edifice!

3. The claim that there is no room for error in our acquaintance with our percepts does not imply the quite distinct claim that we are necessarily aware of all features of our percepts. I can see nothing wrong with claiming that although we are not aware of all the features of our percepts, our acquaintance with the features we are aware of is

such as to leave no room for error. This point is analogous to the common-sense dictum that although a witness may not have seen many features of an event, he can nevertheless be (morally) certain of the character of that which he did see.

NON-FOUNDATIONAL EXPLANATION AND INDUCTION

What is requisite at this point is more development of some key features of a non-foundational epistemology. This is needed to help clarify the role of non-inferential knowledge in a non-foundational scheme, to help fill out the general scheme touched on in Chapter 2, and to clarify the degree of assurance we can have of having any knowledge of the external world. In particular, the exploration of these issues requires some discussion of the processes of explanation and induction. Of course, in a work such as this we cannot delve into these issues in much detail. But an account of the general structure of explanation and induction is clearly needed to help settle these issues.

Explanation

The fundamental goal of all inquiry is the type of knowledge we refer to as understanding, and the process by which we convey understanding is referred to as explanation.

Recall that I have already distinguished between two sorts of knowledge: knowledge by acquaintance and knowledge by description. Reaching understanding by explanation is a species of knowledge by description. That is, explanation is a special sort of description. To make this move,

of course, commits me to the claim that one cannot reach understanding by means of acquaintance. Rather, what is required is a special sort of description.

To clarify this, let me start by considering what Pierre Duhem had to say about explanation:

> To explain (explicate, explicare) is to strip reality of the appearances covering it like a veil, in order to see the bare reality itself.[33]

Despite all the metaphors Duhem used here, it is pretty clear that he is treating explanation as something quite distinct from our acquaintance with percepts. Rather, it is a form of description; but of course, a special sort of description.

In distinguishing explanatory descriptions from other descriptions, I part quite substantially from Duhem's account. He, like others of a phenomenalist bent, would regard non-explanatory description as any description of "appearances" (which, I take it, he would wish to identify with what I call percepts). An explanation would then be a description of the "reality" which "lies behind", or causes, those appearances. This is, of course, a conception of explanation and understanding which has a long history. It goes back at least as far as Plato.

While bearing an important affinity to this traditional view, my own position regarding explanation and understanding departs from it in certain key respects. First of all, I draw the line between "reality" and "appearance" in a way quite different from the more traditional way adopted by

Duhem. I claim that our percepts are part of reality. There are not such things as appearances (if we understand by "appearance" something that is unreal). Rather, there are only incorrect descriptions. Thus the only means by which one could ever "strip reality of the appearances covering it like a veil" would be by replacing incorrect descriptions with correct descriptions.

However, explanation is not simply the replacement of an incorrect description with a correct description. That is, explanations are not simply correct descriptions. We may define explanation in this way:

(P6) Explanation is redescription in terms of more fundamental entities of one's ontology.

This definition requires some further elaboration. We can do this by listing some examples, and by indicating how they fit into this definition:

(a) We explain chemical interaction by describing the electron shells of the atoms of the involved elements, and by appealing to Coulomb's law, etc.

(b) Nominalists explain appeals to kinds by restating them as references to general terms in the language.

(c) We describe a car we see in more detail, mentioning the distribution of chrome, the arrangement of colours, etc.

(d) We explain the falling of a stone by appealing to the universal law of gravitation.

Example (a) is a straightforward case which serves to illus-

trate the definition. The descriptions of the electron shells of the atoms which constitute a given volume of an element are paradigm cases of descriptions which get to a more fundamental level of the ontology than do, say, listings of the various elements and compounds. This example contrasts sharply with example (c), which is a case of a description which does not redescribe the car at any more fundamental ontic level, and hence does not serve as an explanation.

Example (b) is introduced to illustrate two points. First, it shows that the scope of the concept of explanation being defined here is not restricted to scientific contexts. Secondly, it serves to stress that this concept of explanation is not tied down to any given ontology. Thus we can speak of explanations as being either true or false. A true explanation is one which appeals only to actually existing entities, and to true laws of nature. The example I have given here is an explanation which is, I have argued, false. That is, I have argued that there are real kinds which cannot be redescribed in terms of linguistic units. Thus example (b) shows us that we can count as explanations not only cases of replacing incorrect descriptions with correct descriptions, but also cases of replacing what are thought to be incorrect descriptions with what are thought to be correct descriptions. Then only true explanations succeed in replacing incorrect descriptions with correct descriptions.

If the reader wonders why I have bothered to relati-

vize explanation to different ontologies, let me explain that this is done merely to conform more closely with usual English usage. That is, "explain" is used in such a way that it is permissible to say, for instance: "The Egyptians explained the changes in the Nile by reference to the moods of a river-god." Relative to an ontology which included a river-god, the descriptions of the moods of that god did serve to explain the movements of the Nile. However we, of course, believe such explanations to be false. I am not particularly interested in insisting that "explanation" must be used in this manner. If one wishes to stipulate that only what I call "true explanations" may be properly referred to as explanations, I am willing to acknowledge the legitimacy of such a usage.

Example (d) serves to show that the notion "more fundamental" must be construed as implying that any law is more fundamental, ontologically, than are the variables which it relates. This example illustrates a case which is, I take it, a paradigm case of explanation. But it differs from, say, example (a) in that it does not appeal to any objects in the explanandum which do not appear in the explanans. Instead, it appeals to a universal law governing the relations of the objects of the explanans.

In the context of this thesis, a rather obvious question arises: How is the distinction between explanatory description and non-explanatory description related to the distinction between intrinsic description and extrinsic

description? I believe the answer to this question is that the two distinctions are quite independent. Clearly, intrinsic descriptions may serve as explanations: Example (a) provides us with such a case. In that example, the description of the atomic structure of the elements certainly describes the intrinsic nature of the elements.

But extrinsic descriptions can also be explanatory. That this is so is shown by our example (d). That it is an extrinsic description is revealed by the fact that it specifies a type of dynamical relation between a given objects and all other massive objects.

I believe it is also the case that either intrinsic descriptions or extrinsic descriptions can <u>fail</u> to be explanatory. This is due to the fact that for a description to be ontologically more fundamental, it must pick out structures which have important roles in the dynamical laws of the theories which are relevant to the domains being considered. One can always pick out micro-structures or macro-structures, in terms of which to make descriptions, which play no role in the dynamical laws. For instance, example (c) is an intrinsic description, since it describes the spatial distribution of chrome and colours which occur on the surface of the car. But this structure plays no dynamical role in our theories. Hence it does not serve to explain. Similarly, one could extrinsically describe the car as falling in a given place in some pattern of various models, colour schemes, etc. of the auto manufacturer's products.

Again, such a structure could not serve us in providing an explanation.

Induction

This brief discussion of explanation has served as an introduction to the topic that is of most importance in clarifying a non-foundational epistemology: that topic is induction. The goal of the process of induction is, after all, the development of theories, which serve to explain events, and which provide us with understanding. In other words, the process of induction aims to provide us with the account of reality which is the goal of all realist philosophies, and this account of the fundamental ontology of the world must serve to explain events and provide us with understanding.

A discussion of induction naturally divides into two topics: a portrayal of the inductive process, and a discussion of the problems of induction. These latter constitute the problems encountered in trying to justify making the projections we make in adopting our theories of the character of reality. I will first present an account of a non-foundational inductive process. This is meant to be a rationalized account of the actual process of the advance of knowledge.

In the process of induction we are primarily looking for the particular sorts of generalizations which are referred to as laws. These are particular sorts of generalizations, in that there are true generalizations which are not laws,

and true generalizations which are laws. True generalizations which are not laws are referred to as accidental generalizations. The key difference between laws and accidental generalizations is that the former support subjunctive conditionals (an important subclass of which are the counterfactual conditionals), while the latter do not. When I say they "support" subjunctive conditionals, I mean that a statement of the law may be replaced by the corresponding subjunctive conditional with the resulting statement remaining a true statement. The subjunctive conditionals which correspond to accidental generalizations are always false. If they were true, then the so-called accidental generalization would actually be a law.

It must be stressed that this correspondence between laws and subjunctive conditionals does not serve as a means of discovering what, in fact, is a law versus what, in fact, is an accidental generalization. Rather, what it does is serve as a test to determine how we are using the generalization: it determines whether we are using the generalization as a law or whether we are using it as an accidental generalization. Thus when I say that laws are generalizations which are replacable by subjunctive conditionals, I am displaying what we mean by "law" versus what we mean by "accidental generalization".

Some authors have exerted substantial effort in attempting to define a class of statements which are "law-like" in contrast to being accidental.[34] In the case of

Nelson Goodman, the latter class was elaborated to include the generalizations which make use of grue-type predicates. I claim that this quest is in vain. That is, the only sense which can be given to the idea of lawlike generalizations is one which does not serve to distinguish them from accidental generalizations. This is so because we must regard as lawlike any generalization which could, in some possible world, conceivably support subjunctive conditionals. And most, if not all, accidental generalizations meet this criterion. Granted, the conceivable worlds in which our accidental generalizations are laws are very strange worlds, but that is beside the point. Even Goodman's grue-type generalizations are lawlike. The only contrast that can be made that even resembles this contrast between lawlike generalizations and accidental generalizations is the contrast I drew in the previous paragraph between laws and accidental generalizations.

Goodman sought a way of distinguishing lawlike from accidental generalizations so that instances of the former could serve as confirming instances, while instances of the latter could not be confirming. The problem he was concerned with in this context is a problem which also concerned John Stuart Mill, so I will refer to it as "Mill's problem". Mill states the problem in this way:

> Why is a single instance, in some cases, sufficient for a complete induction, while in others myriads of concurring instances, without a single exception known or presumed, go such a very little way towards establishing an universal proposition?[35]

Goodman believes that the proper way of solving this problem is to seek criteria for distinguishing between lawlike generalizations and accidental generalizations, where the former would be amenable to confirmation (by even one instance?), and the latter would not. Goodman elaborated Mill's problem further by introducing his grue-predicates to construct more generalizations whose instances do not serve to confirm them. I am claiming that seeking an elaborate definition of "lawlike generalization" is not a feasible mode of solving Mill's problem. In what follows I will indicate how that problem is to be solved.

First of all, a proper account of the inductive process requires that instances of generalizations be distinguished from confirming instances of generalizations. That is, not all positive instances of generalizations (not even all positive instances of laws) are confirming instances for the generalization. Only those instances of a generalization which serve to test it--viz. to decide between it and some competing generalization--may serve as confirming instances. A competing generalization in a test is a contrary (rather than a contradictory) which is proposed as a law, and the test is sought out to reach a decision between the competitors. That confirming instances are confined to such cases is a lesson of the famous paradox of the ravens: "All ravens are black" is logically equivalent to "All non-black things are non-ravens." But a white piece of paper is an instance of the latter expression. Thus, if all instances

were confirming instances, an observation of a white piece of paper would serve as a confirming instance for the hypothesis that all ravens are black, and we would be led to the absurd conclusion that one could learn something about ravens by looking at blank pieces of paper.

Nelson Goodman does well to note that the observation of a white piece of paper also confirms (in the same, overly broad, sense of confirmation) the contrary generalization that no ravens are black. However, he does not allow this observation to lead him to the proper conclusion. The conclusion that should be drawn from the paradox of the ravens is this: Those statements which are instances of both a given generalization and its contrary are irrelevant to deciding between them, and hence are irrelevant to the inductive method which focuses on instances which are decisive between competing hypotheses. Such is, of course, the type of method I am advocating.[36]

I have been speaking here as though particular generalizations can be confirmed or refuted. But this is not so. Strictly speaking, only theoretic systems are confirmed or refuted. Generalizations are confirmed or refuted only insofar as they are components of confirmed or refuted theoretic systems. Now, the concept of a theoretic system is quite important in the inductive process, so I will try to clarify the concept.

A theoretic system is a system of propositions from which definite predictions may be inferred. (In the context

of confirmation or refutation, the prediction is ultimately to be expressed in terms of properties with which we are directly acquainted in experience, even though for pragmatic reasons this is not usually done.) For this to be the case, the system must include a great many propositions which are usually assumed when we claim to make a prediction on the basis of a single law. Such assumptions include theories of the operation of measuring instruments, theories of perception, theories of mathematical operations, theories of logical operations, and even some metaphysical principles. At the time of writing this thesis, it is not perfectly clear to me the exact range of propositions that are required to comprise a theoretic system. However, one point is clear: the range of propositions is very broad indeed. A good criterion for deciding what range of propositions must be included in a theoretic system is this: If a refutation of a system via modus tollens is not conclusive, then the system is not a complete theoretic system. In other words, the system must contain assertions which would head off any attempt to discount a refuting instance. For example, suppose we consider a system composed of one assertion: "The earth is flat". We predict, on the basis of this simple system, that ships at sea will disappear by simply getting smaller on the horizon. The system is then apparently refuted by observing that ships disappear by sinking below the horizon. But this 'refuting' instance can, in turn, be discounted by claiming that light travels in curved paths. This gives us a more

inclusive system, comprising both the claim that the earth is flat and the claim that light travels in curved lines. Clearly, other assumptions, concerning processes of perception, can also be built into the system. Such an inclusive system would be much harder to defend if a refuting instance were to occur, simply because it leaves less room for manuevering. And if the system is inclusive enough, any refutation of it would be completely conclusive. Then, if any particular part of the system is to be defended in the face of refuting instances, then other parts of the system must be altered--i.e. one must adopt a different theoretic system.

Thus it is a theoretic system which is confirmed or refuted. Confirmation and refutation then occur only when there is a decision to be made between competing theoretic systems. Each system (call them T_1 and T_2) can be used to make a large number of predictions. In actual practice, most of the predictions of competing systems are the same (in the sense that the values of the observables they predict are both within the range of "experimental error"--I will refer to the latter as an "uncertainty-range"). But there are some predictions which differ significantly. These predictions are the ones which are relevant to confirmation and refutation. Thus we have: T_1 implies P_1, and T_2 implies P_2, where $P_1 \neq P_2$. When an observation is made in this context, there are four possible results: O_1, O_2, (O_1 and O_2), and not-(O_1 or O_2), where O_1 is an observation whose uncertainty-range includes P_1 (but not P_2), and O_2 is

an observation whose uncertainty-range includes P_2 (but not P_1). An observation (O_1 and O_2) is, then, one whose uncertainty-range is so great as to include both P_1 and P_2. This sort of observation would not, of course, serve to decide between the systems. An observation not-(O_1 or O_2) refutes both systems, and calls for the immediate production of a system capable of making the proper predictions. Such new systems are often mere ad hoc revisions of the earlier systems. The situations we are interested in are those in which either O_1 or O_2 are observed. If O_2 is observed, T_1 is refuted and T_2 is confirmed. If O_1 is observed, T_2 is refuted and T_1 is confirmed.

Although the refutation of a theoretic system is conclusive, confirmation is never conclusive. Confirmation is merely the passing of one test in an unending series of tests, where refutation may occur in any one of the tests. Now, although the system can be conclusively refuted, this does not mean its components can be conclusively refuted. Any component of the system may be utilized in some new system. Indeed, the usual practice is that when an experiment occurs, we regard ourselves as testing only some isolated element of the system (e.g. a specific quantitative law). But this is a matter of pragmatics. Logically, the entire system is refuted (modus tollens). Pragmatically, we are ready to revise only a small part of the system (e.g. the value of a constant in a particular law). As a general pragmatic rule, we seek to make the least extensive revisions possible in

refuted theoretic systems. This is where the often-discussed element of simplicity enters into the inductive process.

So here is the general picture we get of the inductive process: We possess a theoretic system. In practice, most of the system is not explicitly stated. The system does not succeed in explaining everything we hope it might. In particular, there are anomalies. For this reason, one or more alternative systems are developed. Of course, it is rarely the case that the differences between the competing systems run very deep. Indeed, they usually have very much in common. In any case, those observations are sought which will decide between the competitors. The observations are made, refuting some systems, and confirming others. In practice, since the systems have much in common, attention is paid only to some specific differences between the systems, e.g. one or two generalizations. Although a system may be conclusively refuted, the generalizations on which attention is fixed are not, and advocates of a given hypothesis may make other alterations in their theoretic systems in order to accommodate the observations.

Now it may be asked where these new theoretic systems come from, and it may be asserted that the real inductive process is that which allows us to produce the new theoretic systems. These remarks lead us to introduce the traditional distinction between the process of justification and the process of discovery. However, I would like to recast that distinction as one between the process of justification and

the process of invention. After all, the process of justifying the adoption of theoretic systems includes the process of discovering that the theoretic system is true, or at least discovering that it approximates truth better than any competitor. I am certainly willing to grant that the process of invention is a key component in the inductive process, although I do not regard it as constituting the whole of that process. After all, the invention of theoretic systems would be of no use if we had no method for confirming or refuting them. But I have not discussed the process of invention simply because so little is known about it.[37]

That the account of the inductive process I have given here suits a non-foundational epistemology is evident from the role played in it by observations. Foundational epistemologies attempt to construct theoretic systems from a foundation of observations. I am claiming that such constructions are not part of the inductive process. Rather, the role played by observations is one of testing otherwise-developed theoretic systems. Observations, rather than serving as foundations for theories, serve as touch-stones for discovering the truth or falsity of theories.

Recall that I have claimed that our acquaintance with our percepts is incapable of error. Recall also that I have claimed that observations (the O_i of pp. 120-1) always incorporate a range of "experimental error". It may then be asked how these two claims are compatible. In this way: First of all, it is evident that the physical interaction we

refer to as perception has a degree of indeterminateness to it--discrimination may not occur at any level of accuracy. This indeterminateness is, I believe, a characteristic of all physical interactions, and it finds its expression in quantum mechanical accounts of physical interactions.[38] To defend this interpretation of quantum mechanics here would involve a devotion of much space to a topic which ranges far afield from the path of this thesis. Hence I will not get into such a discussion. But if it is granted that there is this indeterminateness in physical interactions, including perception, we can see that this indeterminateness does not interfere with the claim that our acquaintance with our percepts is incapable of error. The indeterminateness of physical interactions merely leads us to clarify just what it is we can not be mistaken about. What it shows us is that our lack of error in perceptual content is never a matter of being aware of exact values of variables. Rather, it is, at best, a matter of being aware that the values of variables fall within a given range. For example, although we cannot be certain of the exact shade of red of a percept, we can be certain that it falls within a given range of reds. Indeed, the sort of indeterminateness apparently implied by quantum mechanics may well entail that there is in fact no such think as a perfectly exact shade of red.

Let me now indicate how the sketch I have given of the inductive process settles Mill's problem. According to the account given here, the answer to that problem is simple

and straight-forward: The reason why one instance is all that is needed for confirmation in some cases is that these cases are decisive between competing theoretic systems. And the reason why multiple instances do not confirm at all in other cases is that these cases involve generalizations which are not treated as laws in any actively considered theoretic system, and hence their instances cannot serve to confirm them. Thus the basic motive for seeking a distinction between lawlike generalizations and accidental generalizations (a la Goodman) is eliminated, and we are left only with the distinction between laws and accidental generalizations which I introduced at the beginning of this section. We discover what is or is not a law by use of the inductive method I have just sketched.

Any reader who is at all familiar with current discussions of the inductive process will have recognized that the account I have given here of that process owes much to the work of Karl Popper. However, to prevent possible misunderstandings, I will briefly indicate how my position differs from his.

One important difference between Popper and myself is in my use of the notion of "theoretic system". It is only these that can ever be conclusively refuted. Popper often speaks as though individual hypotheses within a system of thought can be conclusively refuted. I believe I have shown this not to be the case. An individual hypothesis (e.g. "the earth is flat") can never be conclusively refuted,

nor can it ever be conclusively confirmed. However, we do have good reason to believe in the truth of the theoretic systems we have confirmed, and this, in turn, gives us good reason to believe in the truth of the components of such systems. I shall attempt to support these claims more fully in the next section.

It is true that at times Popper does allude to the fact that conclusive refutation of a theory cannot be produced.[39] But he does this in the context of regarding a theory in such an instance as metaphysical. This calls to mind another difference between Popper and me: He seeks a sharp distinction between metaphysics and science, while I claim that such a distinction is not to be had. There is not sufficient room for me to defend that claim here.

Also, it is well-known that Popper denies that theories can ever be established as true, and it might be thought on this basis that he denies that the goal of science is truth.[40] And given the similarity of my view to Popper's, the reader might think that I deny that science seeks truth. First of all, I do not think Popper denies the goal of truth for science. For example, he says:

> it is not the <u>possession</u> of knowledge, of irrefutable truth that makes the man of science, but his persistent and recklessly critical <u>quest</u> for truth.[41]

But I do not wish to delve into careful interpretation of Popper's claims here. The point I wish to make is that although one can never <u>establish with certainty</u> the truth of any theoretic system or particular hypothesis, this does not

involve the claim that science (and, I might add, philosophy) does not seek truth. Also, the fact that one cannot establish the truth of a hypothesis with certainty does not prevent us from accepting the best available theories, and reasonably believing them to be true, or at least believing them to be good approximations to truth. The point here is that to rationally accept a theory as true does not require certainty that a theory is true--it merely requires that it is reasonable to accept the theory as true. I am claiming that it is reasonable to accept a theory as true when it has been confirmed in just the sense of confirmation sketched in this section. That this acceptance is reasonable is something I shall argue in the next section.

This stress on confirmation brings up another important difference between Popper and me. He denies that there is any legitimate sense of confirmation, stressing that all that ever occurs in scientific method is falsification. Correlative with this claim, he asserts that his method is deductive rather than inductive, in that falsification is an application of modus tollens. In contrast to these views, I admit a notion of confirmation in my account of the inductive method (recall the distinction I drew between instances of generalizations and confirming instances of generalizations). A confirmation occurs whenever a falsification occurs. And such a confirmation (and, indeed, a falsification of a particular hypothesis) is not a valid deductive inference: it is clearly an ampliative inference, which

carries all the uncertainty of its type. It is also, I might stress, an inference, in that it proceeds from the set composed of the competing theoretic systems and the observed results of a test, to one of the theoretic systems. That is; one infers from the test and its context the acceptability of one of the theories.

Before we move on to a discussion of the reasonableness of the inductive method I have sketched above, it is of interest to note some parallels between the inductive process and the information-processing system between the stimulus$_1$ and the expectation. In the inductive process, an observation contrary to the predicted result refutes an entire theoretic system. In the brain processing, a percept contrary to one's expectation shows that the entire processing system is mistaken. In neither case does the refutation clearly point to a localized error. Also, in the inductive process, we pragmatically focus only on specific parts of our theoretic systems, and alter them, rather than throw out the entire system. Likewise, in the brain processing, our brain efficiently alters only small parts of its processing system at one time.

Since I am advocating physicalism, I claim that this analogy between the inductive process and brain processing is more than an analogy. The inductive process is, after all, the process of human learning; and according to physicalism, human learning is a function of the human brain. Hence key features of the inductive process are the features

of the alterations in information-processing in human brains.

In this context, the following query might arise: If it is the case that only expectations are mistaken or not, then in what sense can we speak of particular hypotheses as being in error or not (false or true)? After all, these particular hypotheses are parts of the system which produces our expectations (predictions). If a part of the system can be a source of the error only equivocally, then isn't it the case that particular hypotheses can be true or false only equivocally? I shall make two points with regard to this query. First I shall argue that it is based on a confusion. Then I will indicate that the problem it raises seems to parallel one raised in the philosophy of physics.

The confusion that is involved here is a confusion between being a source of error and being false. Error and lack of error are our sources of inference to truth or falsity. A part of a theoretic system is unequivocally either true or false. That is, its structure corresponds or it fails to correspond to a real structure. But error or lack of error as a basis of inference to the truth or falsity of a part of a system is equivocal. We can put the distinction this way: Truth is ontological, in that the structures correspond or they do not, as a matter of fact. Error, as a method of inference to falsity, is epistemological, in that it is a method (if equivocal) of reaching knowledge (if uncertain) of truths.

The problem raised above also seems to parallel the

problem in the philosophy of physics as to the conventionalism of geometry and physics.[42] It has been argued that experiments do not reveal unequivocally that Euclidean geometry can be upheld. Thus geometry and physics are presented as a complementary pair. Experiments might rule out a given pair (if the rest of the theoretic system is agreed to by all competing systems), but may not unequivocally rule out a particular geometry by itself, or a particular physics by itself. This claim can be treated as a particular application of my general point concerning confirmation of theoretic systems, and hence also parallels the point concerning the location of sources of error in the system between the $stimulus_1$ and the expectation. As in those cases, one needs also to distinguish here between there being a fact of the matter and the ability of errors to unequivocally lead us to the fact of the matter. Thus it seems that whatever queries may arise in any of these areas will have correlates in the others. I shall rest content here with pointing out this parallel, and leave its further exploration to future research.

The Justification of Induction

Back on pages 93 and 94, we looked at Armstrong's worry over the fact that in a representationist theory of perception, we have only inferential knowledge of external objects. At that point, I indicated that essentially the same problem occurs no matter what one's theory of perception, since all significant knowledge requires projection beyond what is observed. The problems which occur in

attempting to justify our beliefs in such projections constitute the problems of induction. Although there is much reference in the literature to "the problem of induction", there are really several such problems. Consider this list of problems:

(1) Justify making any ampliative inferences at all.

(2) Justify using inductive methods rather than other ampliative methods.

(3) Justify using a particular inductive method rather than other inductive methods.

(4) Justify choosing some particular law rather than others.

(5) Justify making some particular predictions rather than others.

In introducing this list of problems, I have introduced a bit of jargon: I have introduced a distinction between ampliative inferences (or methods) and inductive inferences (or methods). "Ampliative inference" is used here in its usual sense: it is any inference whose conclusion has more content, or conveys more information, than do its premises. As such it is distinguished from non-ampliative inference, which is usually equated with deductive inference. An ampliative method is, then, any method of making ampliative inferences. "Inductive inference" is used here in a more restricted sense. I mean by an inductive inference any ampliative inference which infers the existence of a law in natural phenomena. An inductive method is, then, any method

which aims to discover laws, where a law is any generalization which serves to support subjunctive conditionals. Any inductive method is an ampliative method, but the converse does not hold.

I will briefly sketch how I believe these problems can be solved. But before I do, let me say something to clarify the nature of this enterprise. As with the arguments of Chapter 2 of this thesis, these arguments do not pretend to be indubitable demonstrations of the truth of the conclusions they reach. Rather, they aim to convince, on the assumption any reader of this thesis is likely to share some beliefs in common with the author. Whether such common assumptions are themselves justified would require further discussion which I will not delve into here.

Problems (4) and (5) can be reduced to the earlier problems. That is, we can justify our choice of predictions by appealing to natural laws, and we can then justify our belief in given natural laws by appealing to the methods which led us to that belief. This leaves us with problems (1), (2), and (3) as the crucial problems of induction.

Problem (1) seeks an absolute justification for making ampliative inferences. In this way, it contrasts sharply with problems (2) and (3), which ask for relative justifications. That is, they ask us to justify doing one thing rather than another, while problem (1) asks us to justify doing something at all. Nonetheless, I believe problem one is soluble. It seems to me that an adequate

justification for making ampliative inferences is provided by simply noting that if we are to have any significant knowledge at all, it is necessary for us to make ampliative inferences. It is clear that we could not direct our actions at all if our knowledge were restricted to knowledge of specific past observations. If we are to direct our actions, we must make some sort of inferences concerning unobserved instances, whether we infer by appeal to laws, or by appeal to any other means.

This approach can be strengthened, although in a way which involves us in more controversial claims. I wish to claim that, not only are ampliative inferences necessary for <u>significant</u> knowledge, but they are also necessary for <u>all</u> descriptive knowledge. We can see how this claim can be supported by conducting a brief examination of the role of dispositional terms in knowledge. Dispositional terms always involve, in their use, a projection to cases beyond what is fully manifest, and hence involve ampliative inference (although they are not inferences in the sense of moves from premises to conclusion--perhaps problem (1) would have been more appropriately expressed in terms of <u>projections</u>). But it turns out that far more terms have this projective aspect than those terms which have traditionally been called dispositional. Indeed, I wish to argue that the use of <u>any</u> descriptive term involves projections. For instance, if we say that a certain object is red, we are claiming that if at any other time that object were similarly lighted, etc., it would

appear red. But we can go further than this. Even to say that the object appears red is to say that it possesses a certain similarity with other experiences. We might express this last statement in an obviously dispositional manner by saying that if this appearance-of-red were juxtaposed with other appearances-of-red, they would all look similar. What is essential here is that the use of the descriptive term (even restricted to the "appears" mode of expression) involves a broader reference than to the experience immediately described. In describing an experience by applying descriptive terms to it, we are claiming that the experience has a specific sort of similarity with some other experiences. Indeed, it is this "pointing beyond" which distinguishes describing from naming. We may put all this another way by stating that all descriptions involve the use of terms which denote types, or universals.

If the above argument goes through, then we have a fully satisfactory solution to problem (1). Surely an adequate justification for making ampliative inferences is provided if it is shown that such inferences are necessary for the mere possibility of knowledge. And that is what the above argument aimed to show.

Now we can turn to problem (2). The need now is for a justification of the adoption of inductive methods rather than non-inductive ampliative methods. It is this problem which is probably of the greatest importance of the problems I have listed, since if I can show the way toward its solution,

then I will have eliminated from active consideration any counter-inductive methods. It is these methods which provide the greatest aggravation in the context of the problem of induction, and much attention has centred around trying to show why we ought to adopt inductive methods rather than counter-inductive methods.

Let me first stress the point that counter-inductive methods are not inductive in the restricted sense of "inductive" being used here. It seems that the only candidate for a counter-inductive 'law' one could possibly propose would go something like this: "All apparent regularities will be contradicted at some time". But clearly, this is not a law. Rather, it is a statement about laws, or about apparent laws. And it expresses the crucial fact about counter-inductive inference: it is a method of avoidance of law. Thus it is clearly non-inductive in the stipulated sense.

A key point which will allow us to solve problem (2) is then this: Laws and attributes (including relations) are essentially the same sorts of entities. They are all real regularities, patterns, or structures in nature. That they are the same sorts of entities is indicated by the fact that any of the terms which designate them may be replaced by corresponding subjunctive conditionals. With regard to laws, this is a commonly recognized fact. I have gone so far in this thesis as to adopt it as a criterion to be used in distinguishing between uses of generalizations as laws, and uses of generalizations as accidental. My brief discussion of

dispositionals, where I argued that all descriptive terms must be regarded as dispositional, served to show that the use of descriptive terms also supports the use of subjunctive conditionals (more will be said concerning dispositionals in the next chapter).

From this it is evident that the arguments we applied to solve problem (1) will also serve to solve problem (2). That is, not only does the mere possibility of knowledge require that we make ampliative inferences, but it also requires that we make inductive inferences. The mere possibility of knowledge requires that we project the existence of regularities in nature. This is so because whenever we apply any descriptive term, we are projecting the existence of a real regularity. And given the essential similarity between laws and attributes, we can then say that all description requires the postulation of lawful regularities, and hence, all description is inductive inference.

Once we have justified adopting inductive methods, this leaves us with the problem of justifying the adoption of a given inductive method in opposition to other inductive methods. This is, of course, problem (3). At this point my division of the problems of induction will serve us well. By having eliminated non-inductive ampliative methods from the competition by prior arguments, it is now possible to use a broadly inductive argument to support the adoption of a particular inductive method. What makes this possible, and legitimate, is that all inductive methods share a common

purpose: the discovery of real regularities in nature. Thus we can use this means of choosing between competing inductive methods: Choose that method which has displayed the most regularity in its uncovering of regularities in natural phenomena. In other words, we are to choose that method which has been the most successful in the past in the discovery of natural laws. Of course, the method which fits this description is the method of science, whatever that may be. I claim that the method I described earlier is a rationalized account of the method of science, and hence it is that method we are justified in using.

Now it may be claimed that this solution to problem (3) suffers from the common disadvantage of all inductive justifications of induction--viz. it is circular. But I believe that this is not the case. The justification I have given here is to be distinguished from traditional inductive justifications of inductive methods, since those attempts sought to solve problems (1), (2), and (3) with this single argument. And in doing so, they were legitimately open to the charge of circularity. The crunch of the charge of circularity has always been that non-inductive methods (and especially counter-inductive methods) could just as easily be given non-inductive justifications. But the solution to the problems of induction presented here does not fall prey to this criticism, since it has eliminated the non-inductive methods by independent arguments. The crucial assumption I am now making is that any inductive method will hold this

principle in common with other inductive methods: past success may be appealed to in justifying choices. And by that criterion, held in common by inductive methods, the method of science shows the most success. Thus it is the case that this argument, rather than being circular, proceeds by appeal to a principle held in common among the relevant competing methods.

I believe the arguments I have presented here are sufficient to justify our use of the inductive method I have described, and that method, in turn, justifies our belief in the laws discovered by science. I hope that this discussion has removed any qualms about making claims about the fundamental characteristics of nature, even though it is the case that we have non-inferential knowledge only of some of our own brain-states.

Chapter 6

DISPOSITIONS AND MEANINGS

In the last chapter, when I was concerned with finding solutions to the problems of induction, I made some claims concerning dispositional properties. There I claimed that all descriptive terms must be regarded as dispositional, in that any application of a descriptive term could be replaced by a subjunctive conditional statement. In line with this claim, I also asserted that attributes (which are named by descriptive terms) and laws are essentially the same sorts of entities--i.e. real natural regularities--and that this identity is displayed by the fact that both are dispositional in nature. Thus I equated being dispositional with supporting subjunctive conditionals. In this chapter, I will develop my theory of dispositionals a bit more fully, and place that development in the context of a general theory of meaning. In this context an attempt will be made to settle some ambiguities which have not been clarified by earlier discussions.

DISPOSITIONAL AND MANIFEST PROPERTIES

Here I wish to discuss dispositional properties further, and in particular I want to answer the following sort of objection: If all descriptive terms are dispositional, then how could the dispositional-manifest dichotomy have any

meaning? To answer this objection, I wish to distinguish two ways in which a property may be referred to as dispositional. In one sense, being dispositional is an ontological characteristic of properties. It is in this sense that all properties are dispositional. In the other sense, being dispositional is an epistemological characteristic of properties. In this sense, one can meaningfully distinguish between dispositional and manifest properties. I shall first consider the ontological sense of "dispositional".

As I said before, all dispositional properties are redescribable by use of a subjunctive conditional. (Incidental to the present point, let me note that such a redescription is an extrinsic description). For instance, rather than saying that steel is ductile, one can say that if any steel rod were pulled, it would stretch rather than break. Examples are endlessly repeatable. I have also claimed that all properties are dispositional in just this sense. What I wish to claim now is that this dispositional character is a signal of their ontological status of being real. This parallels the case with laws exactly. If a generalization is used as a law, then it is used to support subjunctive conditionals (i.e. it is dispositional). If it is not used to support subjunctive conditionals, then it is not used as a law: it is not postulated as a real natural regularity. Rather, it is treated as accidental. Properties are, of course, universals, and hence the terms which name them are meant to designate regularities in nature. As with laws,

the support of subjunctive conditionals signals that the predicate is being used to designate a real natural regularity, as distinguished from accidental groupings and arbitrary sets.

Many philosophers have thought that they could do without dispositional properties in their ontology. W. V. O. Quine is a particularly prominent example. In the context of this issue, Quine has done well to point out that science provides us with information about the intrinsic character of many dispositional properties, such as ductility. This is to say that science has provided us with intrinsic descriptions of these properties, and hence has revealed them to be structural in nature. For instance, science tells us that the ductility of a metal is due to the crystaline arrangement of the atoms, and to the forces exerted between those atoms. Prior to such descriptions, the only descriptions of ductility available were extrinsic descriptions. An important example of such extrinsic descriptions is provided by the subjunctive conditional which may replace "ductility". Anyway, science has added intrinsic descriptions to these, by displaying the structures which form the basis of ductility.

However, Quine has the mistaken belief that this type of redescription succeeds in eliminating dispositional properties.[43] But it is easy to see that this is not so. The atoms which serve as relata in the new structure have dispositional properties themselves: e.g. mass and valence.

I discussed mass back in Chapter 3, and there pointed out that it is describable only extrinsically. Most such descriptions are, in fact, subjunctive conditionals: e.g. "If a force of such-and-such a measure were brought to bear on this object, it would have such-and-such an acceleration". Hence mass must, clearly, be treated as a dispositional property. Valence provides another interesting example. Valence is a property of an atom as a whole. To say an atom has a given valence is to say that if it were placed in such-and-such conditions, it would bond to other atoms, forming a chemical compound. Such is, clearly a subjunctive conditional (and hence also is an extrinsic description). So there is no doubt that valence is also a dispositional property. What is interesting about valence is that one can also describe it intrinsically--i.e. its intrinsic character is known and describable. Valence is, intrinsically, yet another structure. It turns out that it is the structure of energy distributions of the electrons of the atom. But here again dispositional properties are not eliminated. Relevant properties here are the electrical charges of the electrons, and the property expressed by the Pauli Exclusion Principle. These are, again, dispositional properties, since they can be easily shown to support subjunctive conditionals.

This dispute over whether dispositional properties are or are not eliminable returns us to the issue between the three sorts of metaphysics discussed in Chapter 3. In

claiming that dispositional properties are not eliminable, I am claiming that the process of analysis of dispositional properties into structures simply produces dispositional properties at the next level down. It seems that Quine could claim that he had eliminated dispositional properties only if he had demonstrated that the infinite hierarchy of structures metaphysic was the proper type of metaphysics--only in that type of metaphysics is every dispositional property analyzable into structures. I will not repeat here my criticisms of that type of metaphysics. Anyway, I claim that at some point one must reach a point where one is dealing with non-structural intrinsic properties. And it is always the case that such properties will be dispositional.

Now we need to clarify the epistemological sense of "dispositional" referred to earlier. It is in this sense that dispositional properties may be contrasted with manifest properties. Properties which are dispositional in this sense are, in an important sense, hidden properties. In particular, they are properties with which we are not acquainted, or of which we otherwise have no information as to their intrinsic character. I suspect that in Quine's discussion of dispositional properties, he was equivocating between this sense of "dispositional", and the ontological sense. After all, it is the case that if one reveals the intrinsic structure of a dispositional property, it becomes manifest, and is hence no longer dispositional in the epistemic sense. But as I argued above, this does not alter the fact that all

properties are dispositional in the ontological sense--i.e. they support subjunctive conditionals.

We can perhaps best defend the claim that the dispositional-manifest dichotomy reflects merely an epistemological difference by displaying an example of a property which is both dispositional and manifest. Consider the property of <u>being grumpy</u>. Clearly, this is a dispositional property in the ontological sense, since the claim that a person is grumpy can always be restated by means of various subjunctive conditionals. And for observers of the grumpy person, that person's grumpiness is also dispositional in the epistemological sense: those of us observing a grumpy person know he is grumpy only by outward signs. We do not experience the intrinsic character of his grumpiness. Rather, we experience only its outward consequences. This is to say that we are not <u>acquainted</u> with <u>his grumpiness</u>. Also, given the limited state of physiological psychology, we also have no intrinsic description of his grumpiness. Thus his grumpiness is not at all manifest to us.

However, the grumpiness <u>is</u> manifest to the person who is grumpy. That is, he is acquainted with his own grumpiness. It is the fact of this acquaintance which makes it sensible to say that <u>he</u> knows how he feels, while others do not. At least, they are not experiencing how he feels. Thus grumpiness is the sort of property which can be both dispositional and manifest.

Now it is often the case that we speak of a disposi-

tion manifesting itself. For instance, we may speak of someone manifesting his intelligence, or dexterity, or whatever. How does this concept fit into the account I've given here of the dispositional-manifest dichotomy? Well, it can fit into our account in three ways. First of all, we can speak of dispositional properties manifesting themselves by their effects. That is, dispositional properties have important roles in dynamical structures, and hence produce other properties in interactions. Some such interactions are perceptual. In these cases, some of the properties produced by the disposition will be properties we are acquainted with in perception. Such properties are, of course, manifest.

Another way in which dispositional properties may manifest themselves is illustrated by the example of grumpiness given above. There we saw that a dispositional property may be manifest, not only in its effects, but also in itself. In this case, those to whom it is manifest are different persons from those to whom it is dispositional. And let me emphasize once more that I am using "dispositional" here only in its epistemological sense.

The third way in which we may speak of a dispositional property becoming manifest is when it is given an intrinsic description, as in the case of Quine's example of ductility. This sort of case of becoming manifest differs from the other two primarily in that it does not lead to the dispositional property becoming manifest to our <u>experience</u>. Here we may speak of it becoming manifest to our <u>knowledge</u>. This seems

to be the primary way in which Quine uses the term "manifest".

ASPECTS OF A THEORY OF MEANING

Before we proceed to a discussion of the problem of secondary qualities, there is a set of issues raised by the discussion to this point that require further elaboration, and need to be tied together more carefully. These issues are primarily semantical, being concerned with the theory of meaning being adopted here.

First of all, I need to indicate the general theory of meaning which is being used here, and indicate how that theory fits in with some of the claims I have made. My theory of meaning may be characterized as a theory of meaning as use. That is, I claim that the meaning of a term is constituted by the roles it plays in a linguistic structure. The adoption of this theory of meaning raises some serious issues which I shall consider in turn.

The Analytic and the Synthetic

The first issue we need to look at is this: How does this theory of meaning fit together with the traditional analytic-synthetic distinction? A particularly biting aspect of this issue is: If meaning is use, doesn't this imply that all scientific laws are analytic, and hence discoverable simply by analysis of the terms involved?

One key to the solution of this problem is the realization that when we make a discovery, we adopt a new use for the term, or for several terms. For instance, when we

discovered that the atomic weight of oxygen is sixteen times that of hydrogen, the term "oxygen" (among others) gained a new use. It already had uses such as: "Oxygen is one of the results of the electrolysis of water", "Oxygen combines with other substances when they burn", and "oxygen is odourless".

How then does this fact serve to solve the problem presented above? It does so by explaining how it is that new discoveries could not be made by examining the already-existing uses of terms. The discovery leads to the adoption of a new use for the term: one which was not there before, and hence which could not be found merely by analysis.

Now it may be objected that when we adopt a new use for a term, we still regard this as a <u>contingent</u> rather than a <u>necessary</u> fact about the referent of the term in question. Another way of stating the objection is this: The theory being propounded appears to imply that once we make a discovery, the new use is adopted and hence becomes part of the meaning of the term. But this claim flies in the face of the fact that these new uses are contrasted with other uses. We say that what is discovered is a (mere) matter of fact, and things could have been different, while the meanings of terms are not matters of fact at all.

I wish to stick to the claim that the meaning of a term is its use, and hence that a new discovery is incorporated into the meaning of a term. However, I do recognize that the stated objection raises a fact about the way we use

terms like "necessary" and "contingent", and this fact needs to be accounted for. We can do this by claiming that analyticity and syntheticity, and necessity and contingency, are essentially pragmatic distinctions. These distinctions are related to a feature of the inductive process: viz. that we always seek minimal changes in theoretic systems, and are very conservative, at least initially, in changes we are willing to accept in our theoretic systems. "Analytic" connections are then those uses which we are least willing to abandon, while "synthetic" connections are the uses that we are more willing to abandon. In this respect I agree with Quine, although we shall see later that I disagree with him in certain other important respects.[44]

Properly speaking, the meaning of a term encompasses all its uses. But we can recognize a progression of pragmatic "layers" in these uses. The outermost layers are termed synthetic, or even accidental. The innermost layers are termed analytic or necessary. Now, it is important that we note that the meaning of a term must be relativized to an individual at a particular time. That is, the meaning of a term, for an individual, is the set of uses that individual would employ that term in, within a given time period. Now, the individual may not use the term within a given time period: thus the subjunctive mood was used in this specification of the meaning of a term. So we can see that the meaning of a term for an individual is a dispositional property of that individual. It is then the fact that for a

group of individuals these dispositions may be very similar that allows us to speak of the language of a culture.

The individual is bound to have some idiosyncratic uses for some of the terms he uses. That there are these idiosyncracies is what leads us to speak of "idiolects". These personal connotations of terms are the stuff of very personal poetry and are the basis of many misunderstandings in communications between individuals. These are the uses of a term which are least likely to be treated as part of "the meaning" of a term. Thus this aspect of the meaning of a term may be placed in the layer which is furthest removed from the uses which constitute the core meaning of the term. In other words, it is these uses which are the most "synthetic".

I must emphasize that the idea of the "correct" uses of a term--i.e. the ones which constitute the established usage; the ones which are "analytic"--is an idea which concerns the pragmatics of the uses of terms. It is quite possible for a person's idiosyncratic use of a term to be incorrect in this sense (i.e. by giving the term a new use not sanctioned by these standards) and yet be correct with respect to truth. That is, the divergent usage may produce a linguistic structure which corresponds more closely with the structure of reality.

Now, if we move further toward the core uses of terms we find recent discoveries which are commonly agreed to but still subject to some degree of doubt. Unbelievers

have refused to adopt the usage and believers would drop the usage more readily than they would many others. Such factors lead us to refer to a use as "contingent" or "synthetic". But still, such uses are more likely to be admitted to be part of "the meaning" of a term than are its idiosyncratic uses.

Still further toward the core meaning of a term we find those uses of a term which have been problematic in attempts to classify them as analytic or synthetic. For instance, we use "colour" and "surface" in this way: "A surface cannot be two distinct colours all over at the same time". The question that has bothered some philosophers is: Is this use of "colour" part of its meaning (is the statement analytic?) or is this merely a contingent fact about colour (and hence synthetic)? I am claiming that such a question is not amenable to being answered unambiguously. Insofar as this <u>is</u> a use of the term "colour", it is part of its meaning. But insofar as it is a use which we might be willing to alter, it is merely contingent.[45]

In the core of these various layers we find statements such as: "Bachelors are unmarried", $2 + 2 = 4$, and $\vdash ((p \rightarrow q) \ \& \ p) \rightarrow q$. Such are uses virtually everyone always agrees to, and which virtually no one would be willing to abandon unless extremely hard pressed. Hence these uses are "necessary" and "analytic".

Now just because I have been claiming that the analytic-synthetic distinction is essentially pragmatic (being

due to the type of committment we have adopted to certain uses of terms), this does not mean that we are free to do what we will in the abandonment or adoption of uses. In other words, it is quite compatible with what I have said to point out that there are good reasons for being unwilling to abandon certain connections. The types of reasons involved are these:

(1) All successful theories have always included these core usage structures,

(2) Abandonment of core structures would pose such deep difficulties that it seems likely that knowledge would fail to advance any further,

(3) Simpler, less penetrating revisions have worked so far, so there is no positive reason to alter the core structures, and

(4) Some aspects of the structure may be built into the brain physiology, so that we may not in fact be able to alter some of our use-structures.

Thus we see that although there is no sharp line to be drawn between the analytic and the synthetic, the continuum of distinctions which underlies that distinction is not without its reasons. However, those reasons do not relate to distinctions between meanings and contingencies of use, but rather between various degrees of our tenacity in maintaining a given use.

I believe that the query which led us into this discussion has now been answered: scientific laws are incor-

porated into the meanings of the terms involved since they are some of the more important uses of those terms. Thus in this sense, such laws can be regarded as analytic. But on the other hand, they are not analytic insofar as they are relatively easily abandoned (relative to principles of logic, but not relative to idiosyncratic associations). The point here is then that the traditional notion of analyticity required the misconception that the meaning of a term is clearly distinguishable as the subset of its uses which could never be altered ("necessary truths"). I am claiming that the meaning of a term is all its uses, any of which could conceivably be altered, but which have varying impacts on our conceptual system if altered. And I will again refer to Quine by noting that he and I agree with regard to the continuum character of the analytic-synthetic distinction. But we disagree in that I do not hesitate to speak of meanings as real. Correlatively, I believe that the meanings of terms in different uses, and the meanings of terms in different languages, are commensurable.[46]

The Uses of Terms and Ontological Committments

How, it may be asked, does this theory of meaning tie into my claims concerning the ontological committments of dispositional terms and statements of laws? We can get into this issue by first stressing that an extremely important set of uses of terms are the referential uses. Under this general category we might include both the language-entry and language-departure transitions spoken of by

Wilfrid Sellars.[47] Indeed, such uses are of such great importance that there is plenty to be said for regarding a language with no such uses as being void of meaning. This realization leads us to further specify the theory of meaning presented earlier by stating that meaning is use in a system which includes referential uses.

For instance: Consider any system of terms such as the ones used in Lewis Carrol's "Jabberwocky". The terms of such a system have fairly definite uses in relation to other terms of the poem. They fit a structure like that of English, with some behaving as nouns, others as verbs, etc. One could perhaps elaborate a substantial grammar for "Jabberwocky", providing rules for moving from one sentence to another (these would be the inference rules). But even with all that, the system would remain meaningless but for the imaginative referents one gives these terms. For a system of terms to be given meaning it must be given some entry and exit transitions. It must, if you will, be connected to reality. And indeed, for the system to be understood, it must be connected to the parts of reality which are our experiences.

Now it must be cautioned that this referential requirement does not apply to each word. That is, it is not the case that for each word to have meaning, it must have a referent. Such is the mistake of the referential theory of meaning, which leads to the complex of problems referred to as Plato's beard. What is being required here is the core of truth we are taking from the referential theory of meaning,

viz. that for the terms to have meaning, there must be some sort of connection to referents. We capture this core of truth by stipulating that the term must have a use in a system which includes referential uses. Clearly, more than one term would have to refer to lend meaning to the system. Just as clearly, not every term of the system need refer. What is required lies somewhere between these extremes. At this time I can add nothing to clarify the limits of these restrictions.

I will develop below the claim that many universal-terms must refer, and that when such terms are used so as to support subjunctive conditionals, referential committments are made. But in this context let me stress that the reason for this claim is not that all terms must have referents, since it is just not the case that all terms must have referents. The reason for this claim is rather in order to have a coherent theory of reality, we must include universals in the ontology; and in order to have a proper account of the use of terms in ways which support subjunctive conditionals, we must treat them as referring. Another caution is required here. I will be arguing below that the use of certain types of terms in certain ways commits one to the existence of certain entities. But it must be stressed that this fact of language does not serve as the grounds for rational belief in the entities in question. After all, linguistic convention can be, and often is, mistaken in its implications. In particular, the following is not to serve as an argument for

the reality of universals. Such existence-claims require other types of arguments, some of which we provided in chapter 2.

Now the claim I do wish to make is that the use of kind-terms does involve reference to real kinds, and that the normal use of such terms commits one to the existence of real kinds. Correlatively, the use of a generalization as a law (i.e. to be willing to interchange it with a subjunctive conditional) commits one to the reality of the regularity. In both cases, when one uses property terms as dispositional, or when one uses generalizations as laws, it is this willingness to replace the disposition or the generalization by a subjunctive conditional which signals the commitment to the reality of the natural regularity. This commitment is what the use of a subjunctive conditional entails.

It is well-known that subjunctive conditionals have been remarkably resistant to reduction within extensionalist or nominalist schemes.[48] I am claiming here that the reason for such resistance is that the proper understanding of subjunctives, dispositionals, and laws requires the realization that what sets these linguistic roles apart from others is that they signal the speaker's commitment to the reality of the regularities which the terms designate. In other words, the regularities are being treated as non-accidental.

Now just because someone uses a term or a generalization in this way does not mean that there really is the regularity he is attempting to refer to. It just means that

the user is implicitly committing himself to the claim that the regularity is non-accidental. He could, of course, be mistaken, as there may in fact be no such non-accidental regularity. So again I wish to stress that the discussion of language here does not, and is not meant to, serve as an argument for the existence of any real regularities. My arguments that there are such were presented in my discussion of universals and my discussion of induction and its justification.

In closing, perhaps I should briefly indicate how my claims here relate to Quine's well-known position concerning the relation between language-use and ontological commitment. That position is that the only way we can involve ourselves in ontological commitments is by our use of bound variables.[49] The context of this claim of Quine's is an argument against the referential theory of meaning. In particular, Quine argues that names are altogether immaterial to ontological issues. I agree with Quine on two counts: (1) In general, the mere use of a term does not involve us in ontological commitments, and (2) the use of bound variables serves as a signal of ontological commitments. However, I disagree with Quine when he claims that the only way we can involve ourselves in ontological commitments is by our use of bound variables. In this section of the thesis I have argued that the use of subjunctive conditionals also involves ontological commitments, and that because uses of dispositionals and laws are replacable by subjunctive conditionals,

so also does their use involve us in ontological commitments.

Chapter 7

THE PROBLEM OF SECONDARY QUALITIES

I have repeatedly indicated that the metaphysical system being adopted here is physicalist (materialist). I have also indicated that this involves adopting the identity theory of the mind. That is, I claim that the mind is the brain, or part of it, and correlatively, I claim that states of mind are brain states. In this chapter I will discuss an important problem confronting any physicalist metaphysics, and confronting any identity theory of mind. That problem I refer to as the problem of secondary qualities.

STATEMENTS OF THE PROBLEM

The problem of secondary qualities, put succinctly, is the problem of clarifying the ontological status of the secondary qualities in a physicalist system. It is the purpose of this section to demonstrate just how knotty this problem is.

The problem arises in its acute form because the physical sciences apparently allow the secondary qualities to play no role whatsoever in the account of physical reality. Consider, for example, colours. According to physics, fundamental particles, such as electrons, are categorically colourless. That is, they are colourless in the same sense as they have no emotions--it is completely inappropriate to

apply any such property to them. Saying that electrons are colourless in this sense is entirely different from saying that they are colourless in the sense of being transparent. A body which is transparent is one which allows light to pass through it essentially unaltered (when the light strikes it at appropriate angles). Also, transparent bodies are those which reflect light in certain ways, have highlights, etc. In other words, their outlines are picked out by a characteristic sort of combination of colours apparently distributed over their surface, these colours being those of surrounding objects. But electrons do not behave in this way at all. They absorb and/or emit electromagnetic radiation of various frequencies. The electron has mass, charge, position, velocity (here I am ignoring the difficulties raised by quantum mechanics), energy, spin, etc.; but no colour.

I have mentioned that electrons emit and absorb electromagnetic radiation. The same could be said, of course, of other elementary particles, atoms, and molecules. Some of this radiation is sometimes referred to as coloured: e.g. we speak of yellow light. But most usually, physicists tell us that such manners of speaking are not to be taken seriously: when we refer to yellow light, we are simply referring to monochromatic electromagnetic radiation of the frequency which, in normal circumstances, causes us to see yellow. Indeed, that monochromatic radiation of a given frequency cannot be said to <u>be</u> a specific colour (eg. yellow) is clearly shown by the fact that experienced colours do not correspond one-to-one with different frequencies of electro-

magnetic radiation. For instance, we can be caused to see the colour yellow by a mixture of frequencies none of which correspond to the frequency of light which would be referred to by a physicist as "yellow".[50] Thus the radiation is also regarded by physical theory as categorically colourless.

The story of colours that physical theory has traditionally given us goes, then, something like this: The sun (among other objects) is a major source of electromagnetic radiation, covering a very wide range of frequencies. A certain narrow band of frequencies of electromagnetic radiation is referred to as "light". Light, whatever its source, whether directly from the sun, from incandescent bulbs, reflected off of various objects, etc., is such that it causes specialized cells in the retina of eyes to produce electrical signals. Some of these specialized cells (rods) signal when struck by light irrespective of its frequency, while other specialized cells (cones) are differentially sensitive to the frequency of the light. This complex mass of signals is processed by the ganglion cells of the retina and assembled into the optic nerve, along which it is sent back to the brain. The end result of all this is the seeing of patterns of colours.[51]

The account I have given here is very crude, but is roughly accurate so far as it goes. Our interest here concerns the processes that are involved in the last sentence above. The philosophical context within which the above sort of account was originally developed was metaphysical dualism.

Hence the brain was seen as being that organ which served to link the mind with the body. In such a context, one could treat colours and other secondary qualities as mental properties. In such a context the percept was regarded as being a mental (non-physical) entity, and its properties were therefore mental properties. Philosophically this was fairly satisfactory. It never allowed any severe problem of secondary qualities to develop, simply because their ontological status was fairly clear: they were mental properties.

However, with the advancing success of neurophysiology in explaining, or at least in presenting reasonable hypotheses concerning, how the brain carries out the "mental" functions, dualism became more problematic. In particular, with neurophysiology's promise to explain, in physiological terms, the activities of human beings, dualism threatens to deteriorate into epiphenomenalism. That is, as the explanatory power of the physical theories increases, the explanatory need for a mind and mental processes decreases to that of a mere repository of secondary qualities, emotional feelings, etc., all of which have, or show promise of eventually having, physiological correlates. The promise is that all the functions of the mind will be entirely explained by reference only to physiological entities, and that the mental entities will be left only as "nomological danglers". The power of this promise is that research in physiology appears to be continually adding pieces to the puzzles it

promises to solve.

Philosophically and scientifically, epiphenomenalism is highly unsatisfactory. In particular, it is hard to believe that there could be a whole range of entities which are entirely extraneous to the natural laws. The nomological danglers of epiphenomenalism have the same sort of status as do singularities in general relativity: they are the sorts of flaws in a theoretical structure which lead one to believe that the entire theory may be seriously mistaken, unless they are eliminable. It makes one suspect that heroic efforts have driven the theory (epiphenomenalism) to the limits of its applicability, and it has now reached the full extent of its explanatory power, and must be replaced by some radical new theory. Singularities in general relativity may well be eliminable: that is not the issue that concerns us here. But the problem of what to do with the nomological danglers of epiphenomenalism is the problem of secondary qualities. In any case, if the problem of the secondary qualities can be solved, the reward is great. The reward is the philosophical success of physicalism, a system of satisfying unity. This would be a sharp contrast to epiphenomenal physicalism, with its apparently extraneous secondary qualities being helplessly carried along by the physical processes. With the problem of secondary qualities solved, the unifying bite of physicalism could then be felt in its full force.

If the nomological danglers of epiphenomenalism

could somehow be shifted coherently into the physiological account, then we would have a clear success for physicalism and, of course, for identity theory. But how this is to be done raises some knotty problems. In particular, the sorts of entities appealed to in neurophysiological accounts appear to be radically different from the sorts of entities appealed to in phenomenological descriptions. The former include "membrane potentials", "ion exchanges", "neural firings", etc. The latter include colours, tastes, attitudes, emotions, etc. How could these disparate properties ever be reconciled? Let me allow another author to express the problem. Consider, for instance, a statement by Ervin Laszlo:

> Regardless of how closely we observe a green patch, and how meticulously we examine our feeling of aesthetic significance, the former will not begin to look the least like a physical wave propogation stimulating the optic nerve in the eye, and the latter will show nothing in common with the sound waves generated by a vocalist singing a Schubert Lied. Conversely, no matter how long and variedly we observe what a physicist takes to be a photon stream, it will not begin to look the least bit green, nor will the wave frequencies produced in the vocal chords of the singer appear moving and beautiful.[52]

I take it that here Laszlo is trying to express a conviction that green and frequencies of radiation are fundamentally different sorts of entities which can never be <u>identified</u>, although they can be <u>correlated</u>. And I should think he would say the same of "neural firings" and "green".

Other authors have also shown awareness of this problem of secondary qualities. J. J. C. Smart portrays the problem in this way:

> It may be possible to get out of asserting the existence of irreducibly psychic processes, but not out of asserting the existence of irreducibly psychic properties... Again, there must be some properties (for example, that of being a yellow flash) which are logically distinct from these in the physicalist story.[53]

In a footnote, Smart attributes this objection to Max Black, and interestingly enough, remarks that it is the most subtle objection he (Smart) has considered, and the one he is least confident of having satisfactorily met. I shall not consider his attempted solution to the problem, since I take it to be essentially similar to Armstrong's attempted solution. I will consider the latter shortly.

As I shall be examining Armstrong's attempted solution to the problem in some detail, perhaps it would be best to look at how he sees the problem. Armstrong seeks to defend what he calls "Central-State Materialism", which is what I have been referring to as Identity theory and, more broadly, as physicalism. His awareness of the problem of secondary qualities leads him to claim that we have no awareness of the "inner" character of our mental states. He seeks to show that we know them only as various types of relations with the environment. Armstrong does this to avoid this sort of objection: According to neurophysiology, brain states and events are electro-chemical phenomena, and include such things as distributions of chemicals, distributions of electrical potentials, firings of neurons, etc. But in our awareness of our own mental states, we are aware of them not as these sorts of states and events, but we are

aware of them as a very different sort of state or event. For instance, according to neurophysiology, the neuron firings, electrical potentials, etc. are not coloured, Yet some of the mental states of which we are aware are distributions of colours. And because these differences are so striking, it is concluded that mental states and events cannot be identified with brain states and events. Such is, of course, the problem of secondary qualities. In reaction to this problem, Armstrong seeks to deny us any awareness of the intrinsic character of our inner states.

The following quotes display the progression of his thought on this topic:

> Central-State Materialism holds that when we are aware of our mental states, what we are aware of are mere physical states of our brain. But we are certainly not aware of the mental states as states of the brain. What then are we aware of mental states as? Are we not aware of them as states of a quite peculiar, mental sort.[54]

Having raised, in these questions, the problem of secondary qualities, Armstrong sketches how he proposes to solve the problem:

> what we mean when we talk about the mind, or about particular mental processes, is nothing but the effect within a man of certain stimuli, and the cause within a man of certain responses. The intrinsic nature of these effects and causes is not something that is involved in the concept of mind or the particular mental concepts.... What it is in its own nature is something for science to discover.[55]

And further on, he elaborates his proposed solution a bit more:

> The concept of a mental state is primarily the concept of a state of a person apt for bringing about a certain sort of behavior... In the case of some

> mental states only they are also states of the person apt for being brought about by a certain sort of stimulus.[56]

The critique of this sort of answer to the problem of the secondary qualities is the topic of the next section.

ARMSTRONG'S ATTEMPTED SOLUTION

It is clear from the quotes provided above that according to Armstrong, we are aware of mental states only by their relations. We might say that they are only known as a structure of relations between the person and his environment. This claim can be emphasized by yet another quote from Armstrong:

> We see the mind as an inner arena identified by its causal relations to outward acts.[57]

It is my contention that it is false to say that we are aware of mental states only by such relations. That is, I think that Armstrong's account of the pre-scientific concept of mind is broadly correct as far as it goes; but I also think that we are aware of more than these relations. In particular, we are also aware (acquainted with) non-structural intrinsic properties of mental states. This disagreement with Armstrong should be made clearer as we look at some specific applications of Armstrong's approach.

Let me start by considering a case where Armstrong and I agree. We both agree that we are aware of the subject of the various awareness relations only by its relations. Armstrong makes this point:

> Because our awareness cannot be an awareness of

> itself, there must always be ultimate awareness which is not itself an object of awareness... This seems to cast light on what philosophers have called the "systematic elusiveness of the subject".[58]

Thus, we are never introspectively aware of the intrinsic character of the subject of awareness, although we do have a relational concept of it: i.e. it is that which is aware, or it is the unscanned scanner. I see no reason to dispute this claim.[59] Of course, Armstrong and I also agree that although one cannot be aware of the intrinsic nature of the subject of awareness through introspection, it is quite feasible that physiological psychology will be able to tell us something of its character.

This example again shows that I do not regard the mind to be "self-intimating". Indeed, there are many other features of our mind that we have no introspective awareness of. All I am arguing in this section is that we have awareness of some intrinsic non-structural characteristics of some mental states. Armstrong denies that we have any such awareness.

It is in Armstrong's account of perception that my disagreement with him clearly shows up. And well we might expect it to over the issue of secondary qualities, since these are perceived qualities. Armstrong's account of perception takes two stages, as he himself points out:

> In the first stage, it is argued that an account of perception can be given in terms of the acquiring of beliefs about the physical world. Many of the traditional problems of the philosophy of perception can be solved at this stage. Such an account, however, must take as primitive the psychological con-

> cept of belief. It is therefore incomplete from the point of view of the attempt to give an account of mental states simply as states of the person apt for the production of certain physical behavior or states apt for being brought about by certain physical objects or situations. The second stage of the argument tries to show that the acquirings of belief involved in perception are susceptible to this sort of analysis.[60]

Thus Armstrong makes it clear that the account he ultimately wishes is a description of perception in these terms: Perception is a process, which is an acquiring of certain sorts of states of a person, these states being described as those apt for... etc. Such is clearly a relational account of perception.

Thus, in the first stage of his analysis of the concept of perception, Armstrong asserts:

> perception is nothing but the acquiring of true or false beliefs concerning the current state of the organism's body and environment.[61]

He then cautions us that these beliefs are sub-verbal. That is, non-speaking children and animals may be said to have them.

It is not long before Armstrong is confronted with certain difficulties that result from this notion of perception. In particular, there is the problem that...

> there are cases where perception occurs, but there is no acquiring of true or false beliefs.[62]

One sort of example Armstrong cites here is the type where we seem to have a perception of something which is not there, and yet we fail to be deceived by the illusion. He uses the example of the image in the mirror as seen by someone who is familiar with mirrors. Another well-worn example

would be that of the stick in the water which appears bent, but the observer, being familiar with the refractory properties of water, does not believe it to be bent.

Now what is especially troublesome in these cases for an approach such as Armstrong's is the fact that there is no false belief involved. In cases where someone is deceived by these perceptions, Armstrong can simply translate the apparent image behind the mirror and the apparently bent stick into false beliefs on the part of those who are having such perceptions. In other words, when someone says "I see someone just like me behind the mirror" or says "That stick is bent", it makes some sense to claim that the perception is the acquiring of a false belief. But what is Armstrong to say to those of us who, familiar with these cases, insist that we do see what looks to be an image behind the mirror, or what looks to be a bent stick, while at the same time we are not led to falsely believe that the stick is bent, or that there is someone behind the mirror?

With reference to these cases, and others like them, Armstrong says:

> All these cases seem to show that we ought to make a distinction between the beliefs that we acquire in perception, and the perceptual experience on which these beliefs are based.[63]

As should be clear from my discussion in Chapter 4, I wish to draw such a distinction. But Armstrong is not willing to make such a distinction because it would lead to the admission that we are aware of the intrinsic character of some mental states (i.e. our percepts). And we have already seen

the danger Armstrong sees in such an admission. The following quote makes it clear that this is, indeed, Armstrong's worry at this point:

> How is this perceptual experience to be conceived? Suppose I have the perceptions that we associate with looking at a red ball. It is clear that I might have had exactly the same perceptions without there being any red ball in physical reality. When we reflect on this point it is very tempting to say that what is involved is some relationship between my mind and a non-physical red-item: a sense-impression or sense-datum. Now it is clear that if there are such items involved in perception, then it is false that perception is simply a state of the person apt for the bringing about of certain physical behavior or a state of the person apt to be brought about by certain physical stimuli. The 'Causal' analysis of the concept of perception would be false.[64]

And, I might add, Armstrong's claim that we are aware of our perceptions only by their relations would be false.

Before I look at Armstrong's attempted solution to this problem, let me remind the reader of two points concerning my position, so there may be no misunderstanding. First, I do not dispute that the "causal analysis" of perception provides us with some portion of the truth. In fact, I think Armstrong's account is a good extrinsic description of perception. What I do dispute is that our pre-scientific awareness of perceptual states is limited to such extrinsic descriptions. Second, although I believe in what may be referred to as sense-impressions, I do <u>not</u> believe that such are non-physical. Sense-impressions (percepts), in my view, are physical states of parts of the brain. How they can have properties which seem to differ so greatly from the properties dealt with by physical science, and yet still be claimed

to be properly referred to as physical, is the burden of my own attempt to solve the problem of secondary qualities in Chapter 8.

Let us now look at Armstrong's attempted solution to this problem of perception without belief. Armstrong's moves are displayed in the following quotes:

> In such cases of perception without belief and even without inclination to believe, it is possible to formulate a true counter-factual statement of the form "But for the fact that the perceiver had other, independent, beliefs about the world, he would have acquired certain beliefs--the beliefs corresponding to the content of his perception."[65]

And he continues at a later point:

> In cases of 'perception without belief', we can now argue, an event still occurs in our mind, an event which can be described as one that would be the acquiring of belief but for the existence of other, contrary, beliefs that we already hold. The event might perhaps be called the acquiring of a potential belief.[66]

And,

> Introspective awareness of such perception would be awareness of the acquiring of such potential beliefs.[67]

These responses to the problem of perception without belief just will not work. Consider the status of potential beliefs. Just what sorts of entities are these? An important feature to note concerning them is that they support counterfactual conditionals. In other words, they are dispositional properties (in the ontological sense). As I argued earlier, their support of counterfactuals is a signal of the reality of the property or state. Thus, a potential belief is not a potential entity (whatever that would amount to). Rather, it is a state which is capable of producing a belief.

Armstrong is apparently not aware that by introducing "potential beliefs", he has introduced "a distinction between the beliefs that we acquire in perception, and the perceptual experience on which these beliefs are based." This is, of course, just what he wishes to avoid. But to avoid this distinction, he would have to argue that a potential belief is a belief, which is absurd. In other words, by introducing these "potential beliefs", Armstrong has introduced entities which are percepts in all respects but name only. It is an actual inner state of the person, of which the person is aware; it is apt to produce a belief, but is not a belief; and the belief it is apt to produce is perceptual. In short, by introducing "potential beliefs" of this sort, Armstrong has, in fact, introduced percepts.

That Armstrong is totally unaware that he is forced into such a conclusion is made evident by this quote:

> The content of our perception, which so many philosophers want to turn into a non-physical object, is simply the content of the beliefs involved.
>
> Our perceptions, then, are not the basis for our perceptual judgements, nor are they a mere phenomenological accompaniment of our perceptual judgements. They are simply the acquiring of these judgements themselves. Our perceptions do not stand between our mind and physical reality, because they are our apprehensions of that reality.[68]

Clearly, Armstrong can make such claims, after his discussion of perception without belief, only if he is willing to identify potential beliefs with actual beliefs. But if he does the latter, then his doctrine of "potential beliefs" cannot carry out the job for which it is designed. After all, it

was meant to account for situations where the beliefs in question are not in fact had as beliefs.

The inadequacy of Armstrong's account of perception without belief can be seen from another direction. Recall that the counterfactual he formulated earlier was this: "But for the fact that the perceiver had other, independent, beliefs about the world, he would have acquired certain beliefs--the beliefs corresponding to the content of his perception." Now what, exactly, is the "content of perception" here? At one point he says "The content of our perception... is simply the content of the beliefs involved".[69] But such cannot be "the content of perception" referred to here, simply because in this case the relevant beliefs are not present. So what else could this content of perception be? It is, of course, the relevant potential beliefs which, I have argued, amount to the percepts Armstrong sought to avoid.

Even if I am correct in claiming that Armstrong's introduction of potential beliefs commits him to the existence of percepts, the battle is still not clearly won. Armstrong might admit that there are such states, and still maintain his claim that mental states are only extrinsically describable, and that we have no pre-scientific awareness of their intrinsic character. Part of my argument against this claim is rather straight-forward: I can provide an intrinsic description of a potential belief (percept). For instance, in the case of the mirror, the intrinsic structure of the percept is describable as that of a reversed image,

located behind the mirror, of the reflected object. But I claim even more than this: I also claim that we are aware of (acquainted with) some non-structural intrinsic properties of the percept--e.g. its colours. Further discussions by Armstrong are directly relevant to this claim, so I will proceed to look at them.

An issue that is closely akin to that of perception without belief is the problem of perception without behavior. That is,

> It seems to be logically possible that some or all of my perceptions should be different from those of other people, yet all my behavior (including speech) be exactly the same as theirs. To take the traditional example, it seems to be logically possible that when I look at pillar-boxes and say they look red to me, in fact my perceptions are what other people would call pillar-boxes looking green to me.[70]

Armstrong then admirably states the problem this poses for him:

> But if we allow this as a logical possibility, it seems that there must be something wrong with the analysis of perception developed in the previous sections of this chapter. For if the content of perceptual beliefs is given by the capacity acquired to behave towards objects in certain ways; and if I and others behave, or have the capacity to behave, towards pillar-boxes and cooking-apples in exactly the same way; then it follows that all of us have acquired the same beliefs about the colour of pillar-boxes and cooking-apples. And if, as we have argued, perception involves nothing but the acquiring of beliefs or the occurrence of mental events that resemble the acquiring of beliefs, it follows that my perceptions and the perceptions of others are identical, which contradicts the supposition of the previous paragraph.[71]

To start with, Armstrong tries to raise some doubts about the possibility of the examples. He notes that red is a 'warm' colour, and green a 'cool' one, and asks if the

reversals occur with respect to warmth and coolness. That is, if the sensed quality red is always warm, then a colour-reversal in perception would result in different statements, and hence different behaviors. But surely it is the case that the warmth and coolness of colours are associations. That is, in experience, red is associated with hot things, such as fires, and green is associated with cool things, such as forest glades. If this is indeed the case, then someone with reversed colour perception would regard his green perceptions as warm (of course, he would call these perceptions "red"), and his red perceptions as cool.

Another question Armstrong raises concerns intermediate colours. Supposing that the speaker's colours are reversed:

> when I say that between red and yellow lies orange, I must mean that between the colour that other people call 'green' and another colour, lies a third, intermediate colour. Now, what are these other two colours? It seems, at least, that there will have to be more than just a reversal of perceptions of red and green.[72]

Yes, more than red and green must be involved. Indeed, what would have to occur to preserve all such relationships would be the replacing of each colour by its complement. In this case, the example of perception without behavior is shown to be quite feasible.

Armstrong notes that C. B. Martin has argued that the trickiest case is one where the population is evenly divided between the two sorts of perceivers, since this would allow neither group to serve as a standard. But to make that

move is to introduce irrelevant issues, such as: Which group is using terms correctly? With regard to the issue with which we are concerned here, the number or proportion of such cases is irrelevant. If even one case is admitted as a logical possibility, then this indicates that we are aware of the content of our experience other than as a set of relations between ourselves, our behavior, and our environment.

There is an important point here which needs to be emphasized. I am not claiming that there will be a difference in content which could not be correlated with any physiological difference. As I am arguing, ultimately, for a physicalist theory of mind, I must insist that any real difference in percepts be constituted by real differences in the physiology of the persons concerned. Armstrong is arguing that we have no awareness of any intrinsic character of these physiological states, and that all such information must be provided by physiology. I take the contrary view that we do have such awareness.

Now let us follow Armstrong's development of this problem of perception without behavior:

> To half the population, pillar-boxes look to be red_1 --which they simply call 'red'. To the other half of the population, pillar-boxes look to be red_2-- which they too simply call 'red'. To the first half, cooking-apples look $green_1$--which they call 'green'. $Green_1$ is in fact no different from red_2. To the other half, cooking-apples look $green_2$--which they too call 'green'. $Green_2$ and red_1 are identical.

The first point to be clear about is that to have, say, a $green_1$ (red_2) perception is not to have

> something green$_1$ (red$_2$) in one's mind. A perception is not a little coloured object. A green$_1$ (red$_2$) perception is a perception, veridical or illusory, as of something in the physical world that has that colour. To have such a perception is, typically, to acquire the belief that there is something green$_1$ (red$_2$) in our environment. Once this point is accepted, it can be suggested that a systematic difference in the quality of perceptions is a systematic difference in the nature of the causes that enable the same behavior in the two different groups.[73]

Then Armstrong offers as a possible systematic differences in causes, the hypothesis that there is a "crossing of wires".

Armstrong admirably states just the sort of objection I would make to his claims quoted above. So I will let him state the objection:

> according to our account of introspection in this book, what one is aware of in introspection is simply the occurrence of a state of oneself apt for the bringing about of certain sorts of behavior. But, if this is so, the fact that the events in one group apt for the discrimination of the colour of pillar-boxes are in the other group events apt for the discrimination of the colour of cooking-apples, and vice-versa, goes for nothing. One group is aware of certain mental events apt for the descrimination of what are publicly called 'green' things. So is the other group. Since this is all that each group is introspectively aware of, on this account of introspection, it follows that each has exactly the same colour-experience. (The 'crossing of the wires' is not an object of experience.) But this is contrary to the supposition that colour-experiences might be reversed relative to each group.[74]

In other words, because of Armstrong's theory of perception, he cannot allow the distinction between green$_1$ and green$_2$, and between red$_1$ and red$_2$. If the behaviours are the same, the experiences are the same (according to his theory).

Now let us see how Armstrong attempts to answer this objection. He does so by presenting us with three possible cases:

(1) A person with crossed wires has his neural wiring put in order, so he then has non-reversed colour perception. But because he associates the term "red" with the colour he perceived pillar-boxes to be before his operation, after his operation he refers to cooking-apples as being "red". That is, his behaviour now differs from that of the rest of us.

(2) Again we start with a person with crossed wires, operate on him to make him like the rest of us, but he chooses to continue speaking as he did before: pillar boxes are referred to as "red". He has carried over this reversal of terminology with the reversal of experience, and is aware of having done so: i.e. he can report his decision to switch words, and can report his awareness of the change.

(3) Again we start with a person with crossed wires, and operate on him to make him like the rest of us. But in this case, he experiences no change at all, can report no change, and continues to behave exactly as before. The following is Armstrong's statement of the conclusion he wishes to draw from these examples:

> The point of considering these three graduated cases is to lead up to the suggestion that in the case where the two sections of a population are equally divided, but their colour-perceptions are reversed relatively to each other, their perceptual experiences, as opposed to their perceptions, do not differ. For, when citizens from a different group have a perception of the sort they both call 'something looking red', then, by hypothesis, they both act, speak, think and feel about the object and the perception in exactly the same way. And if their reactions to the perception are identical, then, ipso facto, their experience of it is identical. There is certainly a

> difference in the mental state itself, but not in the awareness of it.[75]

If I understand him correctly, Armstrong is here asserting that case (3) is the one which would occur. Although he admits that there is a difference between the man with the crossed wires and others, he insists that it is a difference the man cannot be aware of. When the operation is performed, the man is aware of no change. Thus the difference between the two groups of people is confined to a difference in their wiring, and these differences are ones they are not, and cannot, be aware of, short of neurophysiological examination. The difference in perception in the two cases comes down to nothing more than the difference in wiring.

What Armstrong has done here is claim that of the three cases he has listed, the first two are not possible. And this is to admit that the objection quoted earlier is correct--viz. that Armstrong cannot consistently distinguish between experiences $green_1$ and $green_2$, and between experiences red_1 and red_2. He has here admitted that they are identical experiences (in his account of perception). However, rather than proving him wrong, we have only succeeded in forcing him to render his position with regard to reversed perceptions consistent with his general theory of perception.

However, Armstrong's list of cases provides us with a clear way of distinguishing our positions. According to my view, which involves the claim that we <u>are</u> aware of intrinsic properties of our brain (minds), cases (1) and (2) are possible, while case (3) is impossible. According to Arm-

strong, cases (1) and (2) are impossible, and only case (3) is possible. What is interesting about this fact is that it shows that the dispute between Armstrong and me is subject to empirical decision, although such would require more advanced knowledge of physiology than is presently available. But if the requisite knowledge was available, then one would only need to "cross someone's colour-wiring", and see what he reports. If his report corresponds to either case (1) or case (2), Armstrong is refuted. If, on the other hand, the subject's report corresponds to case (3), my view is refuted.

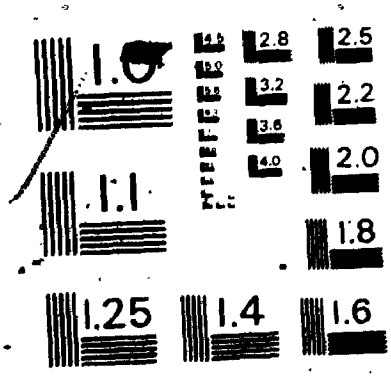
Prior to any performance of such an experiment, it is clear that Armstrong must claim that the concepts we have of colours are, somehow, blank concepts. That is, we are aware of them only by their relations, and have no awareness of their intrinsic nature. If Armstrong were to claim otherwise, then he would have to allow that the results of our experiment would be either case (1) or case (2). The subject of our experiment, if he were aware of colours as non-structural intrinsic properties, would easily be able to report a startling change in his experiences after the operation. Anyway, at a later point Armstrong does claim that colour-concepts are blank. So let us look at what he has to say on this topic.

To start with, he asserts that secondary qualities are properties of the external objects of perception:

> the secondary qualities...are nothing but physical properties of physical objects or processes. Colours of surfaces, on this view, will be simply physical

3 3

OF/DE



MICROCOPY RESOLUTION TEST CHART
NATIONAL BUREAU OF STANDARDS - 1963 A

> properties of those surfaces. And by 'physical properties' is meant the sort of properties a physicist would be prepared to attribute to those surfaces...[76]

But this move lands him in a difficulty similar to that he encountered with mental properties: Most of us seem to think we have some sort of acquaintance with colours. But this acquaintance does not seem to fit into our physical theories of the surfaces, etc. of objects. Here we have the problem of secondary qualities transported into the external world. That is, rather than being a representationist and treating secondary qualities as properties of a percept, Armstrong is a direct realist, and treats them as properties of external objects. I have already criticized Armstrong's direct realism. Now I will criticize his theory of secondary qualities.

Armstrong's attempted solution here is essentially the same as his treatment of mental concepts:

> A mental state is a state of the person apt for the production to certain sorts of behaviour, but the further nature of this state of the person is not given by our concept of a mental state. This blank or gap in the formula enables us to make sense of the assertion that these states are purely physical states of the brain. Physical states can, as it were, be plugged into the gap. Now if we want to make a contingent identification of redness with some physical property, must not our account of what redness is involve some similar blank or gap? There seems to be no other way to carry through a reductive programme...
>
> ... Suppose that our concept of red is <u>all</u> blank or gap? May it not be that we know <u>nothing</u> about what redness is in its own nature? ...Then it would be possible to go on to a contingent identification of redness with a physical property of the red thing.[77]

Thus it is clear that the fact that Armstrong is claiming

that secondary properties are external is irrelevant to the basic dispute between us. Here, as with mental concepts, Armstrong is claiming that, prescientifically, only extrinsic descriptions are possible, and we have no direct acquaintance with the intrinsic nature of these properties. I, on the other hand, claim that although we cannot describe secondary qualities any way but extrinsically, we do have direct acquaintance with their intrinsic character as non-structural intrinsic properties. Armstrong claims colour concepts are all blank or gap, while I claim the contrary. So let us see how Armstrong argues for his position.

He does so by setting up the following imaginary situation:

> Let us suppose that there is an indefinitely large group of people who fall into a number of quite distinct sub-groups: 'families' that do not overlap. Members of the same 'family' all have certain subtleties of feature and behaviour in common that set them off from the members of the other 'families'. Normal observers can be fairly easily taught to sort members of the group as a whole into these mutually exclusive sub-groups. Normal observers spontaneously agree that individuals picked at random belong in a particular sub-group. Nevertheless, because the differences between the sub-groups are very subtle, such observers can make no comment on, and, indeed, have no knowledge of, the way that they sort out these people. They simply sort individuals into groups in a spontaneous way as the result of the action of these individuals upon the perceiver's sense-organs.
>
> Now under such circumstances, I suggest, observers would be entitled to talk about the differentiating properties, and bestow names upon these properties, <u>although they would know nothing at all about the intrinsic nature of the properties</u>.[78]

Now it is of interest to note that this imaginary situation is essentially like the situation chicken-sexers are in, at

least as they are portrayed by Armstrong earlier in his book.[79] Chicken-sexers, according to Armstrong's account, can distinguish male chicks from female chicks, but do so while knowing nothing of the intrinsic differences which enable them to draw that distinction. But then, suppose we ask a chicken-sexer if he distinguishes between red and green the way he distinguishes between male and female chicks? Clearly, he will acknowledge a vast difference between the cases: Right from the start, he could always see that red and green differ; but without much training, he could not see that male and female chicks differ. In fact, we can account for the chicken-sexer's ability only by supposing that there are subtle differences that the perceiver detects subliminally. Such is a clear case where the observer is not aware of the perceptual intrinsic differences relevant to distinguishing the sexes. And as such, it contrasts clearly with his distinguishing between, say, yellow chicks and red chicks, where the observer is aware of the perceptual intrinsic differences. Thus Armstrong's imaginary situation just will not serve as an account of our own awareness of the differences between colours.

What lends Armstrong's claim here the small amount of credibility it has is the fact that colours are non-structural intrinsic properties, and hence cannot be intrinsically described. He treats the fact that we cannot describe the difference between red and green as evidence that we have no awareness of their intrinsic nature. But actually, this

fact is due to the fact that colours are non-structural properties. This is the same as pointing out that they are the simples of experience.

If the example of the chicken-sexer has not served to convince the reader that my position is correct, consider now a difficulty Armstrong is led into--a difficulty for which he asserts he knows no way out:

> Objection 7. If we 'reduce' the secondary qualities of objects to purely physical properties of objects, then it seems that we will not be able to form a coherent conception of a physical object...
>
> I put the argument in the form 'How can we differentiate a physical object from empty space? Mere spatial properties are insufficient, because physical objects share these with empty space. But if we look at the properties of physical objects that physicists are prepared to allow them, such as mass, electric charge, or momentum, these show a distressing tendency to dissolve into relations that one object has to another. What, then, are the things that have these relations to each other? Must they not have a non-relational nature if they are to sustain relations? But what is this nature? Physics does not tell us. It is here that the secondary qualities, conceived of as irreducible properties, are thrown into the breach to provide the stuffing for matter.[80]

Rather than solving this difficulty, Armstrong argues that the secondary qualities could not provide any help:

> ...the problem just briefly outlined must come up for individual electrons. Yet it seems madness to say that the electron has any of the secondary qualities. It would be plainly contrary to what we know of the physical conditions associated with the existence of the secondary qualities. So the problem of non-relational nature must be solved for electrons without bringing in the secondary qualities.[81]

And finally:

> What is the solution to the problem of the non-relational nature of physical objects? I do not know.[82]

Hopefully, the reader will remember that we have already discussed this problem, and indicated how it is to be solved: The physical properties such as mass, electric charge, etc. do not dissolve into relations. Rather, they are non-structural intrinsic properties, for which science has provided only extrinsic descriptions. The fact that we only know these properties by their relations has led some philosophers to the conclusion that these properties are nothing more than their relations. I believe I have shown this view to be mistaken. But what, it may be asked, has all this to do with the secondary qualities? Well, they too are non-structural intrinsic properties with which we have direct acquaintance. So the solution to "objection 7" is this: The non-structural intrinsic properties serve to differentiate physical objects from empty space. Some of these properties are the secondary qualities, with which we are directly acquainted. And because mass, electric charge, etc. are, metaphysically, the same sorts of properties as are the secondary qualities, the way is open for the contingent identification of some of them. But I am getting ahead of myself. The key point here is that my approach offers a solution to "objection 7", while Armstrong's approach does not. And a little later, I will try to show how it could be sensible to ascribe secondary qualities to such things as electrons.

HOOKER'S ATTEMPTED SOLUTION

C. A. Hooker, in his Ph.D. dissertation, The Secon-

dary Qualities and Systematic Philosophy, also speaks to the problem of the secondary qualities. He, like Armstrong and myself, is a physicalist. Like Armstrong, and unlike myself, he holds a direct realist theory of perception (although this is subject to ambiguity, as we shall see). But unlike Armstrong, Hooker believes in a rich phenomenal experience. That is, Hooker does not limit the contents of our perception to beliefs, and believes that Armstrong erred in forfeiting "phenomenological richness".[83] Thus the arguments directed at Armstrong in the previous section are irrelevant to the dispute between Hooker and myself, since we already agree on that issue.

However, there is an important difference between Hooker's approach to the problem of secondary qualities, and my approach. Hooker's solution to the problem of secondary qualities is, essentially, to deny the reality of the secondary qualities. He claims that we are the victims of a systematic illusion when we think that we perceive, say, colours. For instance, he states:

> Colours are not "seen", only objects are seen.[84]

My initial response to such a claim is rather straightforward: If I do not see colours, then surely I cannot be said to see anything; if there is anything which serves as a paradigm case of seeing, it is the seeing of colours. But the trouble with this response is that it really cannot very well count as an argument. So in order to argue against Hooker's position, I need to look more closely at how he develops his

view.

As I have indicated, Hooker's theory of perception claims to be direct realist. Now the basis for this categorization of his theory of perception seems to come down to this: What is perceived is to be understood as the object of a subject-object relationship. But the only subject-object relationship in the process of perception is between the perceiving organism and the external physical objects of perception. Hooker places a great deal of weight on this subject-object relationship by claiming that, although there are inner perceptual states, they are not objects of any object-subject relationship. Rather, they are states of the subject.

I have delineated the difference between direct realism and representationism in a different manner: Whether the subject in the perceptual relation is a transcendental ego perceiving a percept, or whether the percept is a state of the perceiving subject, seems to me to be beside the point. What we need to be concerned with is whether the properties of which we are non-inferentially aware in perception are within our own mind (brain), or whether they are located externally. And the answer to this, as I have argued earlier, is that these properties must be located internally, since our physical theories tell us that the properties of the external world often differ from the properties of our percepts. Now what makes Hooker's position unclear between being direct realist or representationist is that, with

regard to the properties of our perceptions and their identity or non-identity with properties of the external world, he is in agreement with me:

> there is no straightforward identity between what is perceived, even veridically, and the way the world is.85

Hooker places great stress on the fact that the stimuli impinging on us are substantially modified in the process of becoming the data of perception. So, given that Hooker is a representationist in the sense I regard as important, it seems reasonable to regard him as a representationist.

This issue of how one is to distinguish between representationism and direct realism requires further elaboration. What is especially relevant here is a distinction between two ways of characterizing the states within the mind/brain which involve consciousness. On the one hand, these states might be characterized as subject-object relations, where a transcendental ego, or its physical equivalent, is in a special relation to some internal object, where this relation is termed "being conscious of". On the other hand, these states might be characterized as involving no special relation to some transcendental ego, but instead might be characterized as involving merely some additional properties of the internal state. On this latter view, consciousness is an intrinsic property (perhaps structural) of the inner state, rather than being a relation between that state and some special internal entity (the transcendental ego or, in Armstrong's terminology, the unscanned scanner).

Now if I understand Hooker correctly, he takes the distinguishing feature of representationism to be the postulation of an internal subject-object relation, as in the first of the two characterizations of consciousness presented above. Then, since I call myself a representationist, he might claim that I have committed myself to that characterization of consciousness. However, I mean to make no such commitment, at least not at this time.[86] I have used a different criterion for distinguishing between representationism and direct realism. I characterize representationism as the position which claims that the properties of our experience are, internal, and that this is demonstrated by the fact that these properties differ in some respects from what our theories tell us concerning the nature of the external properties which cause our experiences. Direct realism, on this characterization, is the view that the properties of experience are the properties of the external objects.

Now a good part of this difference between Hooker and me is merely a dispute over how we are to use the words "representationism" and "direct realism". There is admittedly a strong tradition behind Hooker's construal of these terms, but I believe that there is also plenty of tradition behind my use of the terms. But rather than explore these traditions, let us look at the substantive issues at hand: Hooker claims that there is no internal subject-object relation; hence I take him to be characterizing consciousness

states in the second of the ways I portrayed earlier. Also, Hooker claims that the content of experience differs from the external causes of the experience. I agree with him concerning the latter point, and adopt a neutral stance with regard to the issue of how consciousness is to be characterized. With these stances on the substantive issues in mind, we can continue with an analysis of Hooker's position.

Since Hooker claims that there is no subject-object relationship within the mind, he needs to introduce some special mode of self-awareness to allow for the awareness of inner states. He refers to a person who is engaging in this special mode of self-awareness as being "selfconsciously-aware" of the appropriate inner state. Here is how Hooker describes that process:

> To have a given inner experience, S say, is to be selfconsciously-aware of the appropriate inner state (or states) arising during the S-experience process; the specific phenomenological character of the S-experience is determined by the character of the inner state (or states) of which the experiencer is selfconsciously-aware at that time;[87]

How do the secondary qualities fit into this scheme? Or perhaps I should ask: How do the secondary qualities fail to fit into this scheme? The continuation of the above quote begins to provide an answer:

> there are no separately identifiable properties of the relevant inner state (or states) corresponding to those terms which we use in the descriptions of those inner experiences.[88]

And later, Hooker states:

> "S's-having-an-impression-of-red" is to be identified with some present state of S.[89]

Evidently then, the relevant states can only be described in some hyphenated manner such as "S's-having-an-impression-of-red", or they can presumably be described physiologically (sometime in the future). In this context, several questions arise immediately: How can an inner state have a character of which we are aware if it does not have properties of which we are aware? Is not a state merely a collection of properties? If the state has properties, but they are not "separately-identifiable", then how can the character of the state be said to be describable?

What I am driving at here, of course, is the claim that these states are describable simply because they are constituted by a collection of properties, some of which correspond to our colour-terminology. At this point, I have little doubt that Hooker would accuse me of adopting the following principle, which he refers to as (P):

> No account of our experience is to be regarded as satisfactory which does not provide an ontologically realized 'archetype' for every phenomenally distinct aspect of our experience; that is, no account of experience is satisfactory which does not provide something which corresponds to every distinguishable feature of our experience.[90]

I readily admit that I am committed to some such principle. So let us look at how Hooker argues against the adoption of this principle:

> The very demand of identity between veridical perceptual experience and what is perceived, which is adopted by most philosophies of perception, is tantamount to adopting P.
>
> But why should this principle be adopted? In isolation it has nothing that I can see to recommend

> it. And I have already rejected the demand for identity between veridical perceptual experience and what is perceived. Why should not the body and/or mind completely determine our secondary quality experience but provide no unique distinct archetype for every distinct secondary quality?[91]

There is a crucial equivocation hidden in this quote. What is equivocal is the sense in which "the demand for identity between veridical perceptual experience and what is perceived" is to be understood. This is equivalent to principle (P) only if "what is perceived" is understood to denote the content of perception: i.e. the perceptual state of which we are selfconsciously-aware (or which is the selfconscious-awareness state). But the identity Hooker argued against earlier is an identity between the content of perception and the external object of perception. As I noted before, I agree with Hooker in rejecting such an identity. But Hooker is now claiming that we are systematically mistaken when we think colours are real properties of the contents of our perception. And the issue here is: Could we be systematically mistaken in this sort of situation? The answer is no. The argument for this answer also provides a good reason for adopting principle (P), or something similar to it.

The essential features of my argument for principle (P) were provided in Chapter 5 of this thesis. There I developed a theory of the concept of 'error'. That theory required that, for a mistake to occur, there must be the

development of expectations, which are then compared with stimuli. Now the way in which this doctrine applies to the present dispute is as follows: If Hooker wishes to claim that we are systematically mistaken in our selfconscious-awareness of our perceptual contents, then it seems that he must claim that selfconscious-awareness involves a complex process of the developing of expectations and comparisons with stimuli (internal). But such a claim is patently absurd and, besides, it seems to be exactly the sort of claim Hooker seeks to avoid in his insistence that there is no subject-object relation within the perceiver. The only alternative to this mess which is available to Hooker, apparently, is to adopt some alternative theory of mistakes. But of course if he does so he must show how that theory is compatible with physicalism. Thus we see that, contrary to Hooker's claim, there <u>are</u> good reasons for adopting a principle such as (P), and the adoption of principle (P) is perfectly compatible with the claim that the contents of perception differ from the external objects of perception.

To help clarify this point, recall that Hooker has committed himself to the claim that we have selfconscious-awareness of some of our inner states. Again, this selfconscious-awareness is <u>not</u> to be understood as a relation between an inner subject and an inner object. Hence it differs from perceptual awareness, which is a subject-object relation, where the subject is a whole person (or perhaps just his central nervous system) and the objects are those of the

external physical world. Hooker has given as an example of a selfconscious-awareness state the state: "S's-having-an-impression-of-red." Hooker also speaks of such states as having a character, but denies that there are separably identifiable properties of such inner states corresponding to the terms we use to describe those inner experiences. In response to this, I am claiming that, first of all, if the state has a character, then it has separately identifiable properties. Secondly, I am claiming that if Hooker wishes to say that these properties do not correspond to the terms we use to describe those inner experiences (that is, if he wishes to claim that these descriptions are mistaken), then he needs some account of how there is room for error within this state. But it seems that the only way to introduce room for error would be to split that state into an inner subject-object relation, which Hooker does not wish to do. Indeed, even if such a relation were introduced, it is very dubious whether that would allow any room for error, since transcendental-ego subjects are not usually portrayed as complexities capable of developing expectations and comparing them with stimuli.

Thus the crucial complaint I have of Hooker's account is the absence of an account of how we could be so mistaken in our descriptions of selfconscious-awareness states as to be led by them to believe in an entire category of properties (i.e. colours and the other secondary properties) which Hooker asserts to be unreal.

Now it may be helpful here to consider a specific example offered by Hooker, and see what can be said about it:

> Consider the following, very crude, analogy: a liquid supports a wave motion in it whose amplitude is varying with time. The total wave motion is to be associated with a visual experience. Now the wave motion can be reduced to the collective motions of the fluid particles and, analogously, the total visual experience can be reduced to an overall physical and/or mental process involving, if necessary, the entire body and mind. But within this former total reduction the phase of the wave and the amplitude of the wave cannot be separated in any simple structural manner, they are both complex functions of the same thing, the totality of particle motions. Nevertheless, phase and amplitude are two conceptually distinguishable aspects of wave motion, and yet there is no sense in demanding individual reductions for them.
>
> In a similar manner I suggest that it is perfectly possible that an entire visual experience should be no more than an internal physical, and possibly mental, process and yet that there should exist no way in which to isolate every conceptually or experiencially distinguishable component of that experience vis-a-vis the process occurring.[92]

This example indicates that Hooker may understand principle (P) differently from me. He seems to interpret it as requiring that each aspect of perceptual content be separable into distinct objects. But I understand (P) differently, or at least wish to adopt it in a different form. I would briefly state the principle in this way: No account of experience is satisfactory which does not provide some property which corresponds to every distinguishable feature of our experience. It seems to me that this statement of the principle does a better job of getting at the dispute between Hooker and me: I am claiming that colours are real properties, while

Hooker is claiming that they are not. In this context, now consider the example quoted above: It is true that the phase and the amplitude of the wave are not separable from the wave; but nonetheless they are real distinguishable properties of the wave. Indeed, a specification of the state of the fluid requires the specification of such properties. Also, it is true that the phase and the amplitude of the wave cannot be predicated of the individual particles which constitute the fluid, but this does not interfere with the fact that the phase and the amplitude are real independently specifiable properties of the system of particles. I suggest that colours could conceivably be treated in an analogous manner: Although it may turn out that colours cannot exist as independent objects, or that they are holistic properties of systems, neither of these factors would lead us to claim that colours are not real independently specifiable properties of certain regions of the brain.

We have yet to consider all the reasons which led Hooker to deny the reality of secondary qualities. I am not going to try to cover all his reasons, but I will consider a couple of others. First, consider what Hooker claims in the continuation of the last quote:

> In visual experience is included geometric properties (shapes etc.), colours and states (of objects). These are the conceptually distinguishable components. For geometric properties and states there are the object's states and properties, which are the individual bases for them. But there are no such bases for colours. The reason lies in the fact that self-conscious-awareness, which is the kind of awareness which we have of colours, is not subject/object in

nature.[93]

Here Hooker is appealing to his claim that selfconscious-awareness is not subject/object in nature to allow him to claim that colours are not real properties. But whether or not selfconscious-awareness is subject/object in nature is, on my view, irrelevant to the issue of the reality of the secondary qualities. If I were to admit that there is no internal object/subject relation, this would not force me (or even incline me) to deny that red (say) is a property of anything. It would merely lead me to say that red is a property of the subject of perception, and is caused by the (intentional) object of perception. It, along with other properties (including the geometric properties Hooker refers to), comprise the brain-states which are our perceptual contents. Let me put this objection another way: Even if self-conscious-awareness is not subject/object in nature, this does not alter the fact that the subject of perception (i.e. the human brain) is a physical object, which has real properties of which we are selfconsciously-aware. Colours are then properly regarded as properties of the subject of perception.

Another set of reasons for Hooker's rejection of the reality of the secondary qualities are these:

(1) the secondary qualities play no role in science,

(2) the secondary qualities convey little or no information about the world, and

(3) the loss of perception of a type of a secondary

quality is never so serious as loss of perception of a type of primary quality.[94]

I will consider these reasons in reverse order: We can quite easily see why the sort of effect appealed to in (3) occurs. The secondary qualities are sense-specific, whereas the primary qualities are not. Hence to lose a type of primary quality requires the loss of several senses, but the loss of a type of secondary quality requires the loss of only one sense. The loss of several senses is obviously more serious than the loss of a single sense. But this fact should not lead us to claim that the secondary qualities are unreal. What it shows us is merely that they must be identified with distinct types of physical parameters which are sense-specific.

(2) is, I believe, false. Consider this: the observed colour is the result of the light-source, the reflecting object, the medium, the eye, etc. Hence it contains, at least potentially, information concerning all these sources. Of course, it is often not easy to take advantage of this information; but this is due to the contingency that often all of the factors are unknown--i.e. we don;t know which contributed what to the final results. But when all but one of the factors are known, the observed colour conveys straightforward information concerning that factor. For instance, one can roughly gauge the surface temperature of a star by observing its colour, provided one has good reason to believe that no other factor is altering the ob-

served colour in any unusual fashion. Odours and tastes also convey information. For instance, the odour of meat tells us whether or not it is edible, and the taste of water tells us if it contains much iron.

At a later point, Hooker plays down the role of colours as aids in our survival:

> the main function of colours being to help as identifiers of objects ("The yellow brick in the corner"...etc.).[95]

But this claim ignores the important role played by colours and other secondary qualities in activities such as food-gathering: Is the apple ripe? Is the fat rancid? Is that the odour of a wild boar? Surely, examples such as these could be listed almost indefinitely.

The truth of (1) is subject to doubt. That is, I wish to claim that its truth is not yet known. Now it is true that nothing called "colour" plays any important role in our physical theories. But the essence of my proposed solution to the problem of secondary qualities, which I shall discuss in the next chapter, is the claim that colours can be contingently identified with certain properties (such as, for instance, electrical potential) which already have a traditional role in our physical theories. If this claim turns out to be correct, then colours will have played an important role in our physical theories for some time: they would merely have been known by a different name.

But even if that sort of identification does not work, we could still explain why colours, although real

properties, have played no important role in physical theories. Colours are, after all, the medium of vision; and vision is a physical process which is rather exotic both in terms of its frequency of occurrance in the physical universe, and in terms of our limited knowledge of its physiological processes. If colours should, for instance, turn out to be holistic properties of complex perceptual beings, then it would not be at all surprising that they play no role in the interactions of simpler bodies, and hence play no role in our physical theories of those simpler bodies. With the development of fully adequate physiological theories of vision, they would then take on a prominent role in the physical laws pertaining to such complex physical systems.

Chapter 8

A PROPOSED SOLUTION FOR THE PROBLEM

My discussions of Armstrong's and Hooker's attempted solutions have, hopefully, made it clear that we can neither get away with denying our awareness of the intrinsic nature of secondary qualities, nor can we get away with denying the reality of the secondary qualities. Thus we are confronted with the problem of secondary qualities in its full force: we need to show how the reality of the secondary qualities, and their perceived character, can be reconciled with our physical theories of the process of perception.

My proposed solution to the problem is very straightforward. Indeed, it is so straightforward that it has been immediately rejected by many theorists. The solution I propose is to identify the secondary qualities with some of the properties referred to in our physical theories. That is, I claim that some of the properties referred to in our physical theories are the secondary qualities under a different name.

Recall that I have asserted that the secondary qualities are non-structural intrinsic properties. Recall also that, as I argued in Chapter 3, our physical theories postulate the existence of some non-structural intrinsic properties, such as electric charge, mass, etc. What I propose

as a solution to the problem of secondary qualities is that the secondary qualities be identified with some of the non-structural intrinsic properties of our physical theories.

Now of course, one cannot arbitrarily identify non-structural intrinsic properties just because they are such. For instance, it would be inadmissable to claim that red is blue, or that blue is sweetness, or pungency, etc. The reason such identities cannot be allowed is that we are acquainted with the intrinsic character of each of these properties and, in particular, we are aware of them as being different.

However, there is no block of this sort to identifying red with, say, synaptic potentials of 5 mv in the striate cortex. (The example is arbitrarily chosen: Just what non-structural intrinsic properties of physiology might be identified with which secondary qualities requires further research in physiology). That there is no such block is due to the fact that our physical theories tell us nothing about the intrinsic character of the non-structural intrinsic properties postulated by those theories. As noted in Chapter 3, all that our physical theories tell us about electrical potentials (for instance) are the effects such properties have in interactions. In short, they provide us only with extrinsic descriptions of such properties.

Thus the claim being made here is that we are directly acquainted with, in phenomenal experience, the intrinsic nature of some of the non-structural intrinsic properties

postulated by our physical theories. And this should not be surprising to any physicalist. After all, we are aware of something in perception, and whatever it is that we are aware of, it must be physical, according to physicalism. What I am doing here is pointing out that, with further advances in neurophysiology, we should soon be able to clarify just which physical properties we are acquainted with. And the arguments I am presenting here aim to clear a conceptual path for such an identification to occur.

Before I continue with the development and defense of this solution to the problem of secondary qualities, perhaps I should point out that the essential features of this solution have been laid out by at least one prominent philosopher: Bertrand Russell presented key aspects of this solution in his Analysis of Matter. In particular, he recognized that we are not aware of the intrinsic nature of the properties postulated by physics, and he recognized that this lack of awareness leaves room for the possibility that their intrinsic nature is not unlike the intrinsic properties of the so-called mental world. The following quote makes evident his view of the situation:

> We cannot perceive a light-wave, since the interposition of an eye and brain stops it. We know, therefore, only its abstract mathematical properties. Such properties may belong to groups composed of any kind of material. To assert that the material must be very different from percepts is to assume that we know a great deal more than we do in fact know of the intrinsic character of physical events. If there is any advantage in supposing that the light-wave, the process of the eye, and the process in the optic nerve, contain events qualitatively continuous with

> the final visual percept, nothing that we know of the physical world can be used to disprove the supposition.
>
> The gulf between percepts and physics is not a gulf as regards intrinsic quality, for we know nothing of the intrinsic quality of the physical world, and therefore do not know whether it is, or is not, very different from that of percepts. The gulf is as to what we know about the two realms. We know the quality of percepts, but we do not know their laws so well as we could wish. We know the laws of the physical world, insofar as these are mathematical, pretty well, but we know nothing else about it. If there is any intellectual difficulty in supposing that the physical world is intrinsically quite unlike that of percepts, this is a reason for supposing that there is not this complete unlikeness. And there is a certain ground for such a view, in the fact that percepts are part of the physical world, and are the only part that we can know without the help of rather elaborate and difficult inferences.[96]

Now I shall elaborate and defend this view further.

CONTINGENT IDENTIFICATION

The identification of properties that is to be carried through by this proposed solution to the problem of secondary qualities is what is normally referred to as a contingent identification. In any such identification, there are issues of both a semantic sort and an ontological sort which must be met. Here I will try to show how these issues can be handled in the context of the identifications being sought by this program.

Semantic Issues

Identity theorists have been consistently dogged by a set of semantic issues. These have included references to the variant meanings of the terms referring to brain-pro-

cesses as opposed to the terms referring to mental processes. There is also the issue of the need for a topic-neutral identification of each of the properties which are to be contingently identified. Here I will speak briefly to these issues, applying the tools developed in Chapter 6.

A very traditional objection to the identity theory has been that if one attempts to carry through the identification, then the mental terms (e.g. "thought", "belief", "colour") will no longer have the meanings they had prior to the identification, and hence the identity theorist will not be talking about the same properties (or processes) at all. In other words, the properties (or processes) he appeals to in the identity would not be the same mental properties (or processes) referred to by the non-physicalist theories.[97]

Now this problem is closely related to the problem I discussed in Chapter 6 concerning the analyticity of scientific laws. There I pointed out that because the meaning of a term is its set of uses, and because the adoption of a new scientific theory involves the adoption of some new uses for some terms, then if we are to speak strictly, we must admit that the adoption of any new scientific law or theory changes the meanings of the terms involved. If this is so, then the adoption of the identity theory involves us in no problems which do not occur in the adoption of any other new theory. Therefore, those who object to identity theory for this reason must also object to the adoption of any other new theory in any domain whatsoever. This presumably, leaves

them stuck with the claim that only the beliefs we have at this time are acceptable, and that our predecessors in science and philosophy who altered their beliefs were engaging in a totally irrational activity. Such, I submit, is an absurd position.

However, there is an aspect of this objection to the identity theory which may yet linger, and may even be expanded in its application to cover all changes of theory in science: If we change the meanings of our terms when we change our theories, doesn't that render the theories incommensurable, and render impossible any rational inductive process?[98] It does not. It does not do so because the change in meaning is only partial. That is, only some of the uses of the term are altered, while many more remain unaltered. It is these unaltered uses which provide the continuity through changes in theories, and which allow the theories to be commensurable. Some particularly important invariant uses are those in logical contexts and those in observational contexts. To help add substance to these claims, I will briefly relate one example of the latter.

Consider the alteration in the use of the term "black" when one moves from a theory in which black is just another colour with the same status as, say, red; to a theory in which black is the absence of colour. Clearly, in such a move, there are some changes in the use of the term "black". For instance, in the former theory, "Black, red, and green are colours" is a permitted use of the term "black"; while

in the latter theory it is not. Similarly, in the latter theory, "Black is the absence of colour" is a permitted use, while in the former theory it is not. But just as clearly, there are uses of the term which remain unaltered. Especially important here are the reporting uses, as in "That is black" said when pointing to a clean blackboard. Such continuities of use provide the basis for the commensurability of theories.

The point made in the last paragraph is directly applicable to the traditional need for a "topic-neutral" analysis of the mental terms which are to be identified with the brain-process terms. After all, what is sought in such "topic-neutral" analyses are uses of the terms which are neutral with respect to the competing theories, and these are simply those uses which are invariant through the change in theory. An important class of such uses are, as I noted before, the reporting uses. With regard to mental terminology, such reports are usually introspective. Uses such as "I am in pain", "I believe..." etc. are left completely unaltered by the adoption of the identity theory. An especially important aspect of the identity theory is that it involves the introduction of new uses for these terms: For instance, a thought can be given a location, or the colour red can be said to be (hopefully, someday) a specific sort of property in our physical theories, with all the uses that occur in these physical theories carried over to it.

The reader may feel that there is a need for further

specification of the theory-neutral uses of the secondary-quality terms. That is, he may seek a clearer topic-neutral analysis of these terms. The specification of the part of the meanings of the terms denoting secondary qualities which are topic-neutral can be accomplished by using a combination ostensive and behavioral account. I think I can best portray this by giving an example: Suppose I wish to specify the meaning of the term 'tickle'. I can do so by going through the following procedure: I say to the subject: "What I mean by 'tickle' is the sensation you have when I pass my fingers lightly over your belly, like so." The ostensive part of this specification is the reference to the sensation being had by the subject.[99] The behavioral part of the specification is the passing of my fingers lightly over the subject's belly, in order to cause the sensation. Also, I can behaviourally confirm success in the ostensive part of the definition by observing the subject's reaction to my tickling. Similar specifications of topic-neutral uses of other secondary quality terms can be accomplished in a similar manner. It should be clear that specifications of uses in this way are perfectly compatible with a wide range of theories of such sensations, including both dualist theories and identity theories.[100]

Although the need for topic-neutral analyses has always concentrated on the mental side of the identification of identity theory, it is also the case that all such issues apply to the physical side of the identification as well.

After all, it is clear that if the type of identification I have in mind can be carried through, then the terms of our physical theories will also gain a set of new uses. In particular, some of them will be permitted uses in contexts which were previously restricted to the mental terminology. Again, such alterations in usage are no more problematic than are any other changes of theory in science, and the fact that many of the uses remain unaltered provides the continuity across the theory-change.

One more point that should be made before we leave the realm of semantic issues is that the manner in which we have developed the theory of meaning here makes it clear that there is no such thing as an invariant observation language. That is, there are no terms used in observational reporting (none that I know of, anyway) which maintain all their uses unaltered through theory changes. However, although we have ruled out an invariant observation-language, we have not ruled out the possibility of a set of invariant observational uses of terms. It is this set of observational uses which comprise the reports of the properties we are directly acquainted with in experience.

Ontological Issues

Once the semantic problems that get in the way of contingent identification have been cleared away, there remain very real restrictions on such identifications. These restrictions are of an ontological character in that they involve issues of whether or not the properties that

are to be identified are compatible in various respects. We already considered one such restriction briefly when we noted that properties such as red and blue could not be identified, since we are acquainted with both of these as non-structural intrinsic properties, and in this acquaintance we are aware of their difference. And as I noted before, this sort of restriction does not interfere with the identification of the secondary qualities and the non-structural intrinsic properties of physics, since we are not acquainted with the latter (so far as physics is concerned).

But there are further ontological restrictions on contingent identification which we must now examine. One such restriction concerns the contingent identification of particulars. This restriction is that the "two" objects must follow the same spatio-temporal trajectory. For instance, the identification of the morning star and the evening star requires that the morning star not appear on days when the evening star appears, etc. What this restriction amounts to is the claim that the extrinsic descriptions of the particulars which refer to their spatio-temporal trajectories must be compatible. And the fundamental criterion for compatibility here is the principle that no single object can be in two places at the same time. In general, when the particulars in question were thought to be distinct, their extrinsic descriptions were such as to pick out different segments of the spatio-temporal trajectory of the single particular which constitutes their identity. Thus incompa-

tibility of trajectory occurs when the extrinsic descriptions of the particulars are such as to locate them at different spatial positions at the same time, with respect to a single reference frame.

Since we are here primarily concerned with the contingent identification of properties, it might appear to the reader that the restriction discussed above is not relevant to our concern here. However, properties do have tokens, and the latter are particulars. So this restriction is relevant. Indeed, one sort of objection to the contingent identification of mental states with brain states involves the claim that there is an incompatibility in spatio-temporal trajectories. For instance, Jerome Shaffer states the objection in this way:

> The physical events which are intimately connected with my particular mental events have some definite location, probably in the brain. This is not to say that they are localized in some small part of the brain or even in a number of small parts of the brain. Perhaps they are spread throughout large parts of the brain; perhaps they are fields; perhaps they include the nonoccurrence of certain events. Nevertheless such phenomena occur not only at some time, but also at some place or places in the brain. However, so far as thoughts are concerned, it makes no sense to talk about a thought's being located in some place or places in the body. If I report having suddenly thought something, the question where in my body that thought occurred would be utterly senseless. It would be as absurd to wonder whether that thought had occurred in my foot, throat, or earlobe as it would be to wonder whether that thought might have been cubical or a micron in diameter.[101]

Now a good part of this sort of objection is semantic, and thus is handled by the considerations I developed in the last section. That is, we have seen that just because a term is

not used in a certain way ought not get in the way of our adopting a new use for it. So just because we have never ascribed locations to thoughts, this ought not get in the way of our starting to ascribe locations to thoughts, so long as the difficulties involved are no more than semantic difficulties. Once this point is clear, the character of the stated objection itself shows that there is really no difficulty for the identification so far as spatio-temporal location is concerned (at least with regard to thoughts). That is, the spatio-temporal trajectories of the particulars which are to be identified are compatible simply because the mental particulars have never before been given any such trajectories, and are hence open to be located wherever the contingent identification says they ought to be located.[102]

An important misgiving which might remain concerning the identification of spatio-temporal trajectories is that the last paragraph dealt with thoughts rather than with secondary-property-tokens. Aren't the latter given definite locations prior to the sort of identification being argued for here? For instance, don't we see colours located in space, and feel pains located in (say) our foot? One thrust of this query was already answered by my arguments for representationism (Chapter 4), since those arguments served to show that such quality-tokens are not located where they appear to be located to a naive realist. And in the context of the traditional representationism held by dualists, the secondary qualities were regarded as mental, and hence were

thought to have no locations. If we were to use that as a starting point for our identification, the identification would not be hindered by incompatible locations, just as was the case with thoughts. Of course, the type of representationism I advocate does ascribe locations to these property-tokens, but they are not the locations the naive realist would give them. And it was my arguments for representationism which served to argue that their location is within the brain rather than in other parts of our bodies or outside our bodies.

Now there is another thrust of the query made above which calls for more involved discussion: Even if it is admitted that the perceived secondary properties are not located where they appear to be to the naive observer, there is still the fact that within the percept (I will here concentrate on visual perception) there are spatial relations which any physiological correlate of the secondary quality-tokens must duplicate. For instance, if we see a red circle surrounded by a blue circle, then according to the representationism I argued for earlier, that means that we have a visual percept in our brain which includes a red circle surrounded by a blue circle. And if the contingent identification being sought here is to have any hope of success, the need for compatible spatio-temporal trajectories requires that there be, in the brain, a circular region of red-correlate surrounded by a circular region of blue-correlate.

Some basic experimental evidence is already extant

which indicates that such spatial relations are preserved by the physiological percept-correlate. First of all, we may note that the best candidate for the location of the visual percept is the striate area of the occipital lobe of the cerebral cortex. One analyst of the experimental evidence, J. J. Sheppard, states:

> The experimental evidence is conclusive for placing the final termination of the afferent signals of the human visual system in the occipital lobe of the cortex. This region is known as the "striate area."[103]

This region is initially chosen for the location of the percept on the assumption that such should be located where the afferent signals of the human visual system terminate. But further reasons for this choice arise when we begin to consider some of the characteristics of this region, comparing them with the characteristics of our visual percept. For instance, consider this further assertion by Sheppard:

> The terminal synapses for the visual process are in the striate area of the cerebral cortex. However, the signal complex there is not afferent determinate, but under the dynamic control of still higher brain centers. Further, the presence of a signal complex in the striate area is not synonymous with the perception of a sensation.[104]

On first reading this statement the reader might think that it refutes my claim that the striate area is the locale of the visual percept. But on the contrary, it serves to aid that hypothesis. After all, any adequate introspective account of our awareness of our visual percept must include such facts as these: at any given time we are not fully aware of the total visual field contained in the percept;

we can consciously shift our awareness to different regions of the percept (as distinct from moving our eyes to focus on some other object); and sometimes the characteristics of what we see are partially determined by what we expect to see. The first of these facts indicates that the conscious perception of a sensation does not include the entire percept at any given time, so if we closely associate the visual percept with a signal complex in the striate cortex, it is not at all surprising that the presence of this signal complex is not synonymous with the conscious perception of a sensation. And the other introspective facts I have appealed to indicate that we should indeed expect the percept to be under the dynamic control of still higher brain centers. Thus we can at least see that it is not unreasonable to locate the percept in the striate area of the cortex.

Once we adopt this hypothesis, then we do find that the internal spatial relations of the percept are compatible with the spatial relations of important aspects of this physiological correlate. Sheppard expresses the relevant data in this way:

> The retino-topical organization of the striate area, i.e. geographical projection of the retina on the cortex, has also been confirmed. Each area of the retina is fairly sharply represented in a corresponding area of the visual cortex.[105]

Given that the retina displays the spatial relations that occur in the percept (e.g. an image of a red circle surrounded by a blue circle), this retino-topical organization of the

cortex shows that it too displays these spatial relations. Thus the distribution of signals in the striate cortex does meet, at least superficially, the criterion of compatibility of spatio-temporal trajectory required for some aspect of it (or some result of it?) to be contingently identified with the visual percept.

Now it may be objected that if the striate cortex preserves the spatial relations which occur on the retinas, this implies that we cannot link the striate cortex with the percept, since there are two retinas, and we have only one non-doubled visual percept. This objection rests on a misunderstanding of what is involved in the retino-topical organization. In particular, the retinal images are mixed on their way to the striate cortex. Careful examination of Figure 3 clarifies how this mixing occurs.[106]

It is the area surrounding the calcarine fissure which I have been referring to as the striate cortex. The circles on the right side of the figure indicate the effects on the visual field (the percept) of lesions at various points along the visual pathway. What is of particular relevance here is the fact that the four quadrants of the striate cortex each correspond to a quadrant of the visual field. Thus the retino-topical organization of the striate cortex does not maintain the duplicity of the retinas.

At this point I can answer more thoroughly an objection which was raised against my theory of percepts back in Chapter 4. That objection involved pointing out that a

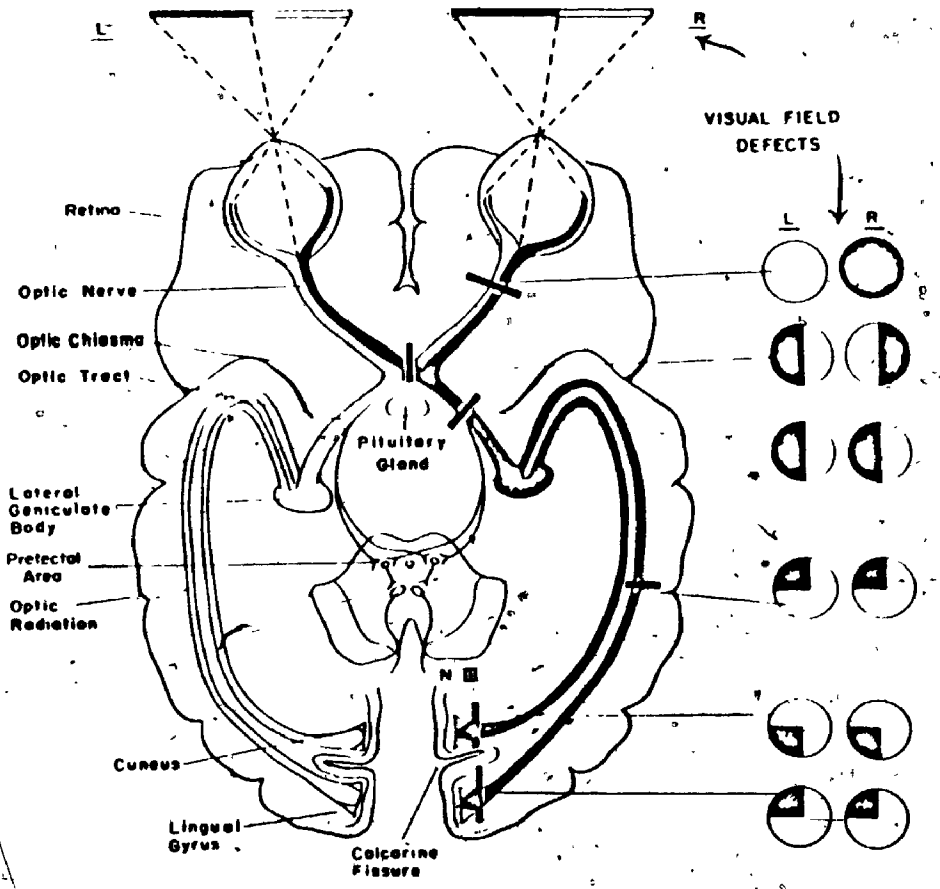


Figure 3

The Visual Pathway

neurosurgeon, when gazing at his patient's striate cortex, cannot see the visual field of his patient. In that context, I stressed that this serves as evidence that the external causes of colours are not the same properties as are the properties which are colours. Then there arose the problem of whether or not the causes of the primary qualities of perception are the same properties as are the primary qualities of perception themselves. Since I claimed that they are the same, this brought up the question: Why then can't the neurosurgeon see those properties in his patient's brain?

Part of the answer to this I have already given: Since we detect primary qualities by means of secondary qualities (in this case, colours), the fact that the surgeon cannot see the colours his patient sees also prevents the surgeon from seeing the shapes which his patient sees. I can now elaborate the rest of the answer to this question: By means of special procedures, the surgeon can see the primary qualities (e.g. shapes) occurring in his patient's percepts. The special procedures are required here to provide another medium for seeing these properties, since the colour medium is not directly accessible to the surgeon. One possible set of special procedures would involve the implantation of numerous microelectrodes in the subject's striate cortex, and then having the wires from these electrodes attached to some means of displaying their firing-pattern (such as an array of lights). Then, due to the retino-topical organization of the striate cortex, if the subject "wired up" in this way were to see something simple, like a circular array of lights against a dark background, the surgeon looking at this special display would also see a circular array of lights. In other words, he would see the primary qualities of the subject's percept (indirectly, of course).

It is worth noting that the crudeness of the sort of procedure outlined above is in large part due to the current lack of knowledge of the exact processes involved in colour vision. If we knew precisely what the physiological correlate of colour was, and had good ways of detecting it, we

could conceivably obtain a much more sophisticated representation of more complex percepts.

I believe that what has been presented so far is sufficient to show that yet another crucial criterion for contingent identification is satisfied. That is, it now seems reasonable to proclaim that the spatio-temporal trajectories of percepts, and parts of percepts, are entirely compatible with the spatio-temporal trajectories of the striate cortex and its parts. Thus in seeking the precise physiological correlate of colour, it seems clear that where we need to focus our attention is in this part of the brain.

However, we have by no means taken care of all the ontological restrictions on the contingent identification of percepts and some aspect of the striate cortex. In particular, we have not fully considered the sorts of restrictions there are on the contingent identification of kinds of entities. (Recall that the spatio-temporal restriction pertained to the contingent identification of particulars). We briefly considered one such restriction when we saw that properties such as blue and red could not be contingently identified, since we are acquainted with them as different. What is yet to be considered is the compatibility between intrinsic and extrinsic descriptions of the properties in question. And given that the sorts of properties we will be attempting to identify are such as to have no intrinsic descriptions, we need only concern ourselves with the compatibility of their extrinsic descriptions.

Now, recall that when we carry through the contingent identification, we are bound to be changing the meanings of the terms involved, as was explained in the section on semantic issues. This means that we will not be leaving all the extrinsic descriptions unaltered through the identification. For instance, if the sort of identification I have in mind is carried through, it would eliminate the description of the property red as a property which plays no role in physical science. So what is required of us is to perform some preliminary sorting of our extrinsic descriptions of the properties involved, deciding which we must keep and which we are to dispense with. I will not attempt here to fully deliniate how that sorting is to be carried out. Rather, I will just indicate some of the extrinsic descriptions which we may not eliminate in this sorting, and which must be found to be compatible if the contingent identification is to have any hope of success.

On the physiological side of the contingent identification we have properties such as electrical potentials. All of the physical laws in which these play a part must remain unaltered by the identification. That is, all the positive claims of the physical sciences are to remain intact through the identification. What will be eliminated are various negative claims which are interpretations placed on the physical sciences to place them in the context of other beliefs, e.g. "electrical potentials are entirely different from colours."

On the phenomenological side of the contingent identification we have, for instance, colours. The extrinsic descriptions which must be preserved across the identification include claims of sameness (e.g. "both those patches are the same shade of red") and difference (e.g. "that patch is greener than this one"). Included here are descriptions of, say, the limitations of our abilities at matching patches of the same brightness but differing hues, in contrast to our ability to match patches of the same hue but differing brightness. When we carry through the contingent identification, it is descriptions such as these which must be preserved across that identification:

Researchers in the field have displayed an awareness of such requirements. For instance, consider this statement Cornsweet makes concerning the example I last mentioned:

> If a subject is shown two flashes, both at 510 mm, one flash delivering 1,000,000 quanta to his eye and the other 2,000,000 quanta, he will call them both green. The effects of these two flashes on his visual system are not identical, since he could be shown to be capable of discriminating between them; but there must be some aspect of these two effects which is the same if, under the experimental conditions, he makes the same response to both; that is, if he generalizes between the two flashes.[107]

Such is the sort of requirement I have in mind when I say that if we see two patches as being the same in some respect (phenomenologically), then their physiological correlates must be the same in some respect (physiologically) if the patches are to be contingently identified with that physiological correlate.

For the identification to be successfully carried

through, all extrinsic descriptions of these sorts must match completely. At this time, there is a fair amount of information available concerning how the stimuli at the eye are processed so as to produce the appropriate characteristics by the time the signals reach the striate cortex. However, this information is far from being complete. Also, I have yet to run across any attempt by any researcher to actually identify the physical property which is the perceived colour (perhaps this is due to a pervasive implicit belief in epiphenomenalism?). Rather than survey all these results, I will briefly indicate how these sorts of tasks can be (and are being) carried through by looking at one characteristic: the production of the same subjective hue from different wavelength mixtures in the incipient radiation.[108]

Recall that in the last chapter I pointed out that no colour-hue can be identified with electromagnetic radiation of a specific wavelength, since the same colour-experience can be produced by a mixture of wavelengths which includes no quantity at all of the wavelength previously "identified" with that colour. For instance, one will ordinarily see yellow if light of 578 nm strikes the retina, but one will also see exactly the same shade of yellow if a suitable mixture of other wavelengths strikes the retina. Such a mixture need contain no light of 578 nm. What is needed, then, to allow for the possibility of a physiological correlate of colour which might be identified with it, is a demonstration that exactly the same signal reaches the striate cortex in

these two cases (and other similar cases), and an account of how these different inputs lead to the same signal.

Such an account can be given, at least with regard to its key features.[109] Basically, the account rests on the discovery that humans are trichromats--i.e. that we have three colour-systems, where each colour-system responds most strongly to a different distribution of wavelengths (there is some overlap between these distributions). Thus light of a specific wavelength (say, 578 nm) will effect colour system A most strongly, colour system B less so, and colour system C hardly at all. Exactly the same total effect can be achieved by picking the appropriate intensities of other wavelengths so that the colour systems are effected in the same proportions as they are by the single wavelength 578 nm. Then, to complete the account, there must be some mechanism whereby the proportionate effect on the three systems is "put together" in some manner. Cornsweet describes a possible mode of this transformation, although he notes that there is not yet confirmation that this system actually occurs in humans.[110] All that is required is that the receptors in each of the colour-systems have outputs roughly proportional to the logarithms of the rate of isomerization of the pigments in the receptors, and that the colour-systems link up pairwise, with one of the pair exciting the synapse and the other inhibiting it. There is independent evidence that such logarithmic responses of receptors do occur.[111]

In any case, the physiological information briefly

outlined here, and similar information much too extensive to deal with here, indicates roughly how researchers have at least got the beginnings of correlations in physiological properties for the colour-hues of our phenomenological awareness. However, lest there be some misunderstanding, one point needs to be stressed: Accounts such as those given by Cornsweet are best understood as providing models of the type of circuitry required to produce the same signal for the same phenomenal hue. And "same signal" here amounts to the same frequency of neural firings distributed in the same way across a group of neurons. I do not wish to claim that such distributions of neural firings are to be identified with the phenomenal colour-hue. Rather, I am claiming that it is what this signal complex produces in the striate cortex which is to be identified with the colour-hue. The study of the mechanisms which transform the signal which produces this property is relevant to the contingent identification, as it is assumed that the character of the neural signal is very significant in determining the character of the produced property. Thus correlations between the descriptions of these mechanisms and the descriptions of the phenomenal properties do have significance in supporting an eventual contingent identification.

A SPECULATIVE CORRELATE FOR COLOUR

To help clarify what has gone before, it may be of help to speculate a bit here on just what property it is that

is produced in the striate cortex which we are acquainted with as colour of a particular hue. I must emphasize that this is to be little more than speculation, since it is still the case that very little is known about what actually goes on in the human striate cortex. But so that the speculation will not be completely in the dark, I will attempt to fit it into what is known about the cortex.

An important type of activity which occurs in the cortex is electrical: neural fibers enter the striate cortex along the optic radiations, and synapse there. At the synapses, electrical potentials are built up, or are inhibited, until a potential is reached which induces firing of post-synaptic neurons. Now there are two basic sorts of processes going on here which need to be distinguished. Karl Pribram draws the distinction in this way:

> ...distinguish between two types of neuroelectrical activity: nerve impulse unit discharges on the one hand, and graded slow potential changes on the other. Only nerve impulses are propogated; graded changes wax and wane locally in the brain tissue...[112]

What I wish to focus on here as a possible correlate for colour are these "slow potentials". Further points made by Pribram concerning these potentials are of particular interest to us:

> The present emphasis is on the slow potentials themselves--the suggestion is that the slow potentials produce patterns which serve a function in addition to a role in impulse transmission: the view taken here is that the slow potential pattern 'computes' both the spatial neighborhood interactions among neural elements and, to some extent, the temporal interactions over a range of sites by a continuous (analogue) rather than a discrete, all-or-none (digi-

tal) mechanism...

> Thus ephatic and synaptic events, those which are composed at the junctions between neurons, form a pattern. Inferences about the nature of such a pattern can be made from the known fine structure of the brain and the electrical activity recorded from it. Several such inferences suggest that these patterns make up wave-fronts...113

Pribram, at a later point, places these "slow potentials" in the context of the totality of operations of the brain:

> Coding operations take place in the nervous system continuously. Physical energy is sensed by receptors and transformed into nerve impulses. These impulses, in turn, reach synaptic networks where the discrete signals become coded into the form of, i.e. encoded in, the slow potential microstructure. Before this encoded representation can be of influence elsewhere in the brain a decoding operation must take place, nerve impulses must again be constituted and in such a fashion that previously encoded information is not lost.114

Thus the role played by the slow potential patterns appears to correspond quite nicely to the role played by percepts in perception: They are the result of a substantial degree of sorting and encoding, but they then play an important role in causing further activities.

If indeed the slow potential patterns of the striate cortex do comprise the visual percept, then we should expect different aspects of these potentials to correspond to hues and intensities of such percepts. Taking a clue from Pribram's assertion that the potentials form wave-fronts, we can guess that perhaps the frequencies of these wave fronts are the hues of the visual percepts, and that perhaps the amplitudes are to be identified with the brightnesses of the percepts. Again, this last move is little more than a mere guess, and

much more research is required before such assertions can be made with any more confidence.

Anyway, we can see that an electrical field potential meets one fundamental ontological requirement for contingent identification: our physical theories assert nothing concerning the intrinsic character of electrical potentials. This leaves open the possibility that we are acquainted with that intrinsic character in visual perception, and that that intrinsic character is what we refer to as experienced colour.

If we suppose that this identification of electrical potential with colour is correct, then we can appeal to it to explain exactly how it is that the external property which causes us to see colour differs from the internal property which is colour (recall that the presence of this difference was inferred from the fact that the neuro-surgeon cannot see the colours of his patient's percept). When I speak here of the external cause of colour, I am referring to the properties of external objects which we refer to when we say, e.g. "That ball is red". (The contrast here is with "that ball looks red"). This property is what causes an object to absorb certain frequencies of light, or to reflect or emit other frequencies of light. Physics tells us that this property is composed of the differences between discrete energy-levels of the electrons of the surface (or near-surface) molecules of the object in question. That light of a given frequency is absorbed is due to that frequency providing the unit of energy required to shift an electron to a higher

discrete energy level. That light of a given frequency is emitted is due to an electron's having dropped to a lower energy level, in the process of which it emits a quantum of light of the appropriate frequency. Thus the "objective colour" of a physical object is the property which determines which wavelengths are absorbed and which are emitted, and that property is the set of differences between the discrete energy-levels of the electrons of the surface-molecules of the physical object.

Actually, even this property does not correspond exactly to the ordinary notion of the objective colour of an object. This is due to the fact, noted earlier, that many different distributions of wavelengths of light produce the same colour-sensations in humans. Thus a more careful specification of the property which is objective colour would look something like this: A specific objective colour is any of the various sets of differences between energy-levels of electrons of surface-molecules which would produce a distribution of wavelengths (when bombarded with light composed of an equal distribution of all wavelengths) which would produce identical signals in a trichromatic visual system such as humans possess. This is, indeed, a forminable expression, but it does approach what is referred to by "the real colour of an object." In any case, the point here is not so much to get an exact specification of that property, but rather to point out (what should now be obvious) that this property is not simply an electrical field potential. And it is the

latter which I have speculated might actually be what we refer to as subjective colour.

ABOLITION OF THE PRIMARY-SECONDARY QUALITY DISTINCTION

If the general type of program developed here turns out to be correct, then it clearly implies that we must recast the traditional distinction between the primary qualities and the secondary qualities. In the first instance, that some sort of revision might be needed was signaled by the discovery that some properties which have been traditionally classified as primary (e.g. mass and charge) are in a general respect the same sort of properties as are some properties which have been traditionally classified as secondary (e.g. colour and odour). They are the same sort of property in that they are all non-structural intrinsic properties.

But in the second instance (and this is definitely the more significant move), the type of program developed here postulates that some of these primary qualities are identical with some of these secondary qualities. In particular, I have speculated that colour is an electrical field potential. And the fact of these identities requires not merely that we revise the distinction between the primary and the secondary qualities, but that we abolish that distinction. After all, we cannot place a single quality in two mutually exclusive classifications and expect our classification scheme to make any sense. But such is just what we are doing when we refer to electrical field potential as a primary quality, and refer

to colour as a secondary quality (assuming my speculative identification of these properties to be correct). Instead, we must place properties like mass and charge under the same classification as we place properties like colour and odour. And in doing so, we abolish the distinction between the primary qualities and the secondary qualities.

This by no means leaves us with no system for the classification of types of properties. What we are left with is the far more meaningful distinction laid out in chapter 3: the distinction between relational properties, structural intrinsic properties, and non-structural intrinsic properties.

Finally, we can note once more that if the approach taken here is correct, then the intrinsic character of the non-structural intrinsic properties of physics is no longer mysterious. Once we reach the realization that properties such as mass and charge are not relational, but are really non-structural intrinsic properties describable only extrinsically, we are no longer confronted with the difficulty of distinguishing matter from empty space (a problem which, as we saw earlier, befuddled Armstrong). And once we further realize that some of these properties are the properties we are directly acquainted with in experience, then we need no longer regard their intrinsic character to be a mystery forever beyond any hope of solution.

Appendix

EXPECTATIONS AND PURPOSES

Perhaps a fruitful way of exploring the concept of expectation will be to provide a couple of examples which lie somewhere between the extremes of a stone, which cannot be said to have expectations, and a human being, which clearly does have expectations. We can consider two simple examples; one animate, and the other inanimate.

Consider a moth. It is an often used example that moths will fly into hot lamps, etc. when attracted by the light, and will unwittingly injure or destroy themselves. Now I do not wish to suppose that moths have any self-conscious awareness of what they are doing, but it does make sense to say that the moth did not expect to be injured or destroyed. So it seems that this example is one in which we have an organism simpler than a man, which has an expectation, and which has that expectation thwarted. Hence the moth can be said to be mistaken.

I am not here concerned whether moths do or do not fit the description of them given here. What I am concerned with is the concept of an organism which engages in purposive behavior, and which has its expectations thwarted. This type of organism is being conceived as being different from man not only in degree, but also as being qualitatively different insofar as it is incapable of learning from its mistakes.

(I expect that here we might be departing significantly from the case of real moths). Thus I am claiming that a learning ability is not required of an organism if it is to have expectations and make mistakes, although it is the case that learning adds a significant new dimension to the process. Thus we need to look for a clarification of 'expectation' elsewhere than in the learning process. Our next example should help us in this task.

Consider now a mechanism composed of a thermostatic relay and an internal refrigeration unit, as in Figure 4.

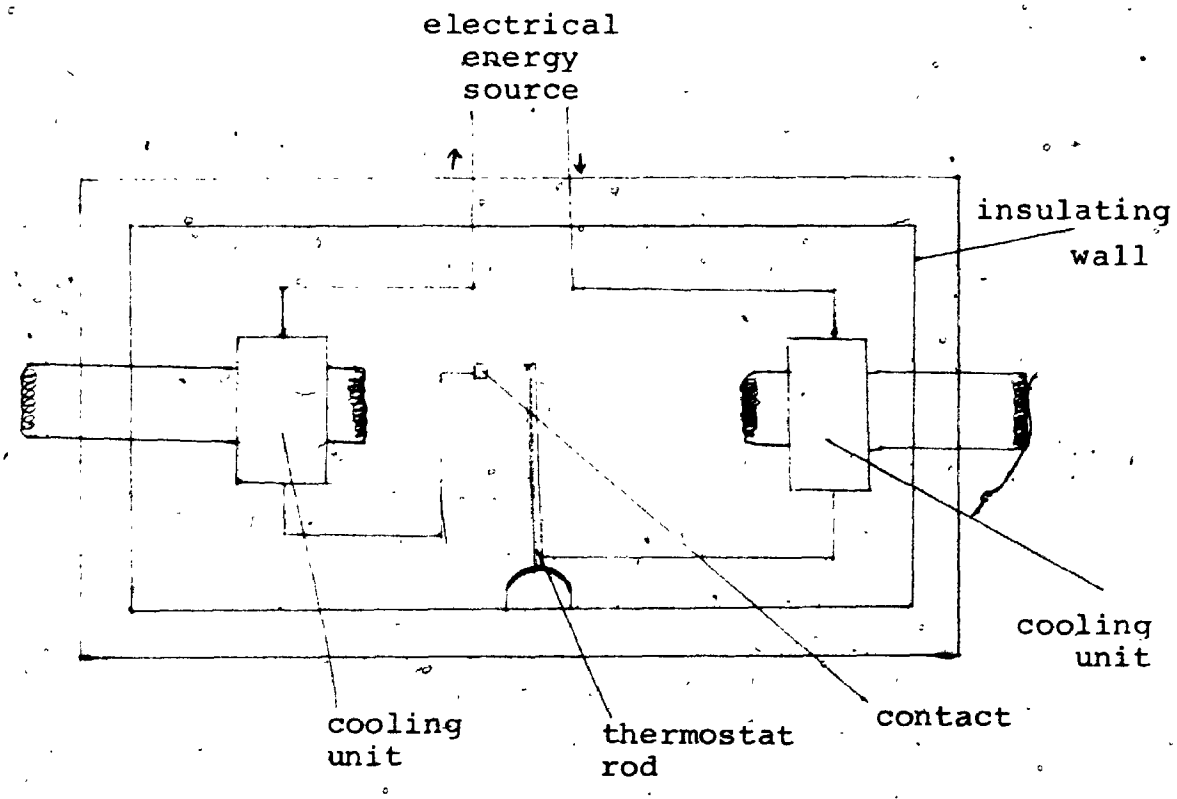


Figure 4
A Primitive Mechanism with Expectations

We can envision this device as located in an environment which is hot enough to melt its more delicate components, so that the only factor which is preventing its destruction is the periodic running of its cooling unit. Suppose also that if the cooling unit were to run continually, the mechanism would break down because of excessive cooling. Finally, to make this mechanism independent, suppose that it produces its own electricity from, say, external radiation.

The mechanism of Figure 4 is obviously far simpler than that of even a mot . Yet I wish to claim that it can be said to have expectations, and hence can possibly be mistaken, in exactly the same sense as does our mythical moth. It, like the mythical moth, is a non-learning mechanism. This example is simple enough, I believe, to allow us to sort out the essential elements of what constitutes an expectation.

In describing the moth, I spoke of it as exhibiting purposive behavior. The mechanism of Figure 4 also displays purposive behavior. It interacts with its internal environment with the purpose of maintaining a specific temperature range. I will try to make clear just how purposes play a role in the concept of expectation.

First of all, we need to point out which processes of our mechanism correspond to which parts of Figure 2 (see p. 94). The "skin" of Figure 2, dividing the internal from the external world, clearly corresponds to the insulating wall of Figure 4. The entire external world sequence of Figure 2 then corresponds to the maintanence of the hot environment of

Figure 4. The stimulus$_1$ is then the internal temperature produced by the external environment. The internal causal sequence is the contact of the thermostat and consequent activation of cooling units. The expectation is that of a lowering internal temperature. The comparison between expectation and stimulus is carried out by the thermostat, simply by reacting to the temperature, and either contacting or not. It is as though at every instant of time the thermostat is asking: Is it cool enough? The answer comes in the form of how far the rod is bent. If the rod touches the contact, this constitutes a "no" answer. Let me stress that I am not proposing that there is any consciousness on the part of the device. The only form of awareness which is displayed here is the rod's response to temperature change. Thus, in a rudimentary sense, the rod is 'aware' of the temperature in its immediate environment. But of course, the rod is not self-aware at all.

The purposes of the mechanism are built into its structure. Clearly, the purpose of the cooling mechanism, thermostat, etc. is to maintain a given temperature. We could also say that values are built into the structure--i.e. the mechanism values a given temperature range.

Can we construct a situation in which this mechanism can be said to be mistaken? Yes we can. One such situation is this: Suppose that there is a being in the environment which can protect our mechanism from heat, or can blast the mechanism with so much heat that its cooling system cannot

keep up with it. Suppose also that this external being responds to the thermostat's activities, so that if the thermostat does not turn on at its usual temperature, the external being will cool the mechanism after it reaches a higher, but non-lethal, temperature. But if the thermostat turns on, then the external being will destroy it with a blast of heat. Clearly, if the environment includes such a perverse beast, it would be a mistake for the mechanism to turn on its cooling system. Of course, because our mechanism is such a simplistic creature, it will make that mistake (unless it malfunctions). But this does not alter the fact that for the mechanism to pursue its usual course of action would be a mistake.

In light of the examples we have examined so far, it appears that some basic features required by an organism, if it is to be capable of having expectations, include these:

(1) The organism tests some feature of the environment, and alters its behavior in response to the environmental condition. The internally produced factor in this comparison may be termed an expectation. The externally produced factor may be termed a stimulus.

(2) The response of the mechanism is designed to bring the stimulus more in line with the expectation. That is, the response is purposive.

What we have here is what the science of cybernetics

refers to as a negative feedback loop. We can now see that the presence of negative feedback loops are important in determining that an entity is the sort which can be said to make mistakes, have expectations, and be purposive. So we can now say that the reason a stone can never be mistaken, does not have expectations, and does not engage in any purposive behavior, is that it has a structure which contains no negative feedback loops.

As I mentioned before, the learning process adds a new dimension to all this. In particular, it allows for the correction of mistakes. It does this by allowing for alterations in the causal sequence of Figure 2. Allowing for such alterations requires new, much more complex mechanisms and, above all, requires some degree of self-awareness. That is, the structures (in complex beings, these structures are referred to as "conceptual") which are involved in the production of the expectations must be accessible to the scrutiny of the organism, in order to be altered to produce more appropriate expectations. The complexities involved in the addition of the dimension of learning require difficult analysis, and are of great interest. However, I shall not pursue a study of them here, since such a study is not required for the basic elucidation of the concept of expectation which we sought here.

Perhaps a bit more should be said about purposes, in order to avoid any misunderstanding of the doctrines being urged here. As I have said, a purpose is built into the

mechanism of Figure 4, and can be said to be built into any mechanism with negative feedback loops. Organisms which are capable of learning may also have built-in purposes, but they also have the capacity to adopt and abandon purposes. Let us look at an instance to help fill out these claims: The purpose which is built into the mechanism of Figure 4 is the maintaining of a given temperature range. The relevant built-in mechanisms are, of course, the thermostat and the cooling units. The specific temperature range which is purposed is built into the thermostat, in the physical properties which determine at what temperature the contact is made. Now the key difference between a case such as this, and the case of a learning organism, is that in the latter, the structures which embody the purposes of the organism are maleable. The learning organism alters these structures in response to the production of mistaken expectations.

In order to further refine our analysis of the concept of expectation, a distinction needs to be drawn between internal purposes and external purposes. Perhaps we can best display this distinction by looking at two examples. As an example of a mechanism which displays an internal purpose, we can refer again to the mechanism of Figure 4. A similar mechanism, which has only an external purpose, is the ordinary home thermostat heating system. The difference between these two cases is that the activities of the mechanism of Figure 4 are directed to its own survival and well-being, whereas those of the home thermostat are not. The home thermostat

has a purpose, but it is someone else's purpose: the thermostat is designed to produce comfort for the residents of the house. But the mechanism of Figure 4 does not fulfill external purposes in its operation. Granted, I provide it with an external purpose in designing it to illustrate these points, but its functioning does not aim to fulfill that purpose. Rather, its functioning aims to fulfill the purpose of insuring its own survival.

The distinction between internal and external purposes allows us now to clarify the difference between a mistake and a malfunction. That these differ was illustrated earlier when I spoke of our mechanism making a mistake provided it did not malfunction (this was in the presence of the perverse external being). In that situation, a malfunction would have prevented a mistake. Now, an ordinary home thermostat can malfunction, but cannot be mistaken. To be capable of being mistaken, it must have an internal purpose. Mistakes are constituted by an organism's doing something which goes against its internal purposes, but which involves no malfunctioning. Thus we need to add to our earlier account of expectations, to take into account the distinction between internal and external purposes. That is, it is not sufficient that there be a purpose involved: the purpose must be internal. So it would be wrong to claim that any mechanism with a negative feedback loop is capable of mistakes. Indeed, an ordinary home thermostat heating system is a system with a negative feedback loop. But as we have seen, such a mechan-

ism is incapable of making mistakes. So now we must assert that, for a mechanism to have expectations, and hence to have the capacity to be mistaken, it must have a negative feedback loop whose purpose is internal.

Now it may be objected that even in the case of what I have called internal purposes, an external source is required to implant that purpose. For instance, the mechanism of Figure 4 had to be designed and built in such a way as to have its purposes. Thus, it might be argued, no inanimate object can develop such a purpose on its own: There is always a need for a transcendent designer. Hence physicalism cannot be complete.

What this objection ignores is the fact that Darwinian evolution--the process of natural selection--should be able to produce such mechanisms. That such can occur is due to the peculiar nature of the purpose of survival. In a system in which there are entities which reproduce themselves, those which will survive to reproduce still more of their kind would be those which have mechanisms which help to ensure their survival. That is, in a population of various sorts of entities, the ones which are most likely to be around later are those which have internal mechanisms which help insure their survival. And as I have shown, any organism which contains mechanisms which operate to ensure the survival of the organism are purposive organisms, which have the capacity to develop expectations, and hence have the capacity to be mistaken. Thus one would expect that over an adequate

period of time, there should come to be many such organisms in existence.

I do not pretend that the above account of how purposes might develop out of non-purposive matter is anywhere near being complete. Rather, I have attempted only to indicate the general direction such an explanation might take. A fully adequate explanation is far beyond the scope of this work.

NOTES

1. David Hume, An Inquiry Concerning Human Understanding (Bobbs-Merrill, 1955), pp. 84-89.

2. George Berkeley, Three Dialogues Between Hylas and Philonous, (Bobbs-Merrill, 1954), pp. 58-59.

3. This seems to be what Gilbert Ryle wishes to do when he argues that dispositions ought to be interpreted as "inference tickets". See, for example, his The Concept of Mind (London, Hutchinson's University Library, 1949), pp. 117-125.

4. In fact, to say that no such reduction is possible is one mode of expressing the fact that induction is an ampliative inference -- i.e. that induction projects beyond what has been experienced. Also, the general claim that the use of subjunctive conditionals carries an ontological committment is discussed further in Chapter 6.

5. This sort of projection will find further application in my discussion of induction (see Chapter 5).

6. Further arguments of this type are presented by D. M. Armstrong in his A Materialist Theory of Mind (London, Routledge & Kegan Paul, 1968), pp. 104-107.

7. Panayot Butchvarov, Resemblance and Identity (Indiana University Press, 1966), p. 7.

8. ibid., p. 7.

9. This type of nominalism, which is often referred to as extensionalism, is well represented in the works of W. V. O. Quine. Limitations of space prevent me from entering into a general critique of Quine's views here.

10. Bertrand Russell, The Problems of Philosophy (Oxford University Press, 1959), p. 55.

11. Yet another argument against the resemblance theory is provided by Panayot Butchvarov, op. cit., chapter 3.

12. One particularly clear presentation of his views on this distinction occurs in Russell, op. cit., chapter 5.

13. D. M. Armstrong presents arguments similar to these in his Perception and the Physical World (London, Routledge & Kegan Paul, 1961) Chapter 15. It should be noted also

that there is a type of physical theory which embodies this sort of metaphysics, yet which does not fall prey to the problem raised by Armstrong and myself. Geometrodynamics is this type of theory. It skirts the problem simply by denying that there is any distinction to be made between the structures of empty space and material structures. In the program of geometrodynamics, the material properties are identified with spatial properties. Thus the problem raised here against the infinite hierarchy metaphysics cannot arise: We cannot acuse the theory of failing to account for a distinction (between matter and space) which it denies outright. In any case, I am not inclined to take geometrodynamics very seriously, since it is (at this time) primarily a speculative theory, lacking the wealth of confirmation attained by (say) the special theory of relativity or quantum mechanics.

14. Here again geometrodynamics deserves some comment. If that theory could be regarded as the empirically confirmed which comprises the content of current physical theory, then the argument I have given here for accepting the form-and-content metaphysics would not work. Geometrodynamics incorporates an infinite hierarchy of structures metaphysics. However, as I noted before, geometrodynamics has not been empirically confirmed. Hence I am not yet willing to regard it as a part of current accepted physical theory.

15. This idea was even applied, by the Formalist school of David Hilbert, as a stipulation for the development of rigour in mathematical reasoning.

16. That this is a rewarding of a standard presentation of the theory of groups can be seen by comparing it to (e.g.), I. N. Herstein, Topics in Algebra (Ginn & Company, 1964), p. 26.

17. Albert Einstein, Relativity (Methuen & Co., 1920), p. 220.

18. A. N. Whitehead advocates the interpretation I have favored here. See, for example, his The Concept of Nature (Cambridge University Press, 1955), Chapter IV.

19. John C. Graves, The Conceptual Foundations of Contemporary Relativity Theory (M.I.T. Press, 1971), p. 128.

20. Ernst Mach is particularly well-known for such claims. See, for example, his The Science of Mechanics (Open Court, 1960), p. 264-274. Also, D. M. Armstrong, who is by no means a positivist, fell into the same error. See, for example, his Perception and the Physical World (Routledge & Kegan Paul, 1961), p. 188.

21. Berkeley makes this sort of move in his Three Dialogues Between Hylas and Philonous (op. cit.) p. 85. I might also note here that Berkeley's criticisms of representationism are not effective against a system such as mine, since his most powerful arguments are based on the assumption of dualism (as a starting point for his arguments). It is the dualist presuppositions of his opponents which allows him to be effective when he appeals to a principle such as: "Only Ideas can be like Ideas". There is no reason why any physicalist should accept such a principle.

22. I first heard this example used by R. L. Gregory at a conference on perception at the University of Western Ontario in 1974.

23. Discussions relevant to such notions appear in the presentation of my general theory of error in chapter 5, and in the Appendix to this thesis.

24. This is a paraphrased version of an argument presented to me by C. A. Hooker. He attributed it to D. M. Armstrong, but I have been unable to locate the source.

25. D. M. Armstrong, A Materialist Theory of Mind (op. cit.), p. 217.

26. ibid., p. 218.

27. ibid., p. 220.

28. ibid., p. 221.

29. ibid., p. 217.

30. D. M. Armstrong expresses just this sort of misgiving in his Perception and the Physical World (op. cit.), p. 30.

31. Given that much of the information-processing system of the brain is not subject to our conscious control, this raises the possibility that there are errors produced by these areas (in the sense that alterations in them could lead to the production of correct expectations), and that we could not eliminate these sources of error short of mutation or surgery. A related problem arises from the fact that the stimulus which is compared with the expectation (i.e. $stimulus_2$ of Figure 2) is a percept, and hence is the result of a substantial amount of processing itself. This processing is also not subject to our conscious control. There is also room for error

here (again in the sense that alterations here could eliminate errors), so it is conceivable that the production of stimulus$_2$ would allow an expectation to be confirmed which would in other circumstances cause problems for the organism. These possibilities do not lead to the conclusion that we can know nothing of external reality. Rather, they serve to stress the fallibility of that process, and hence should help make us wary of overconfident assertions concerning that reality.

32. C. D. Broad draws essentially the same distinction I have drawn between external and internal purposes, although he refers to "external teleology" and "internal teleology". See his The Mind and Its Place in Nature (London, Routledge & Kegan Paul, 1925), p. 83.

33. Pierre Duhem, The Aim and Structure of Physical Theory (trans. P. P. Wiener, New York, Atheneum, 1962), p. 7.

34. The problem of clarifying how to draw this distinction is what Nelson Goodman refers to as "the new riddle of induction". He struggles with the problem in his Fact, Fiction, and Forecast (Bobbs-Merrill, 1965). The problem is also discussed by Karl Hempel in Aspects of Scientific Explanation (New York, The Free Press, 1965), pp. 70-73.

35. John Stuart Mill, A System of Logic (London, Longman Group, 1970), p. 206.

36. Nelson Goodman makes the move referred to here in his Fact, Fiction, and Forecast (op. cit.), p. 71. The type of method I advocate here is also advocated by P. K. Feyerabend in, for instance, his article "Explanation, Reduction, & Empiricism", p. 66. This article can be found in H. Feigl & G. Maxwell (eds.), Minnesota Studies in the Philosophy of Science, v. III (Minneapolis, Univ. of Minnesota Press, 1962). Israel Scheffler also introduces this approach when he introduces the notion of "selective confirmation". See his The Anatomy of Inquiry (New York, Alfred A. Knopf, 1963), p. 289.

37. We should note that the process of invention itself seems to be divisible into two aspects. On the one hand there is the logic of discovery, as discussed by N. R. Hanson in his "The Logic of Discovery" (Journal of Philosophy, vol. LV, no. 25). Such a logic considers criteria for the reasonableness of considering a hypothesis. (This contrasts with the logic of confirmation, which considers criteria for the reasonableness of accepting a hypothesis). On the other hand, there is the psychology of

invention, which is concerned with the general laws which govern how, in fact, creative inventions are produced. Such is the topic of Arthur Koestler's The Act of Creation (Pan Books, 1970).

38. An interesting defense of this sort of interpretation of quantum mechanics can be found in Michael Audi's The Interpretation of Quantum Mechanics (University of Chicago Press, 1973).

39. Karl Popper, The Logic of Scientific Discovery (Harper & Row, 1968), p. 50.

40. ibid., p. 33, and p. 278.

41. ibid., p. 281.

42. Discussions of this issue may be found in: Hans Reichenbach, The Philosophy of Space and Time (New York, Dover, 1958); Adolf Grunbaum, Philosophical Problems of Space and Time (New York, Alfred A. Knopf, 1963); and Hilary Putnam, "An Examination of Grunbaum's Philosophy of Geometry", in B. Baumrin (ed.) Philosophy of Science: The Delaware Seminar, v. 2 (Interscience Publishers, 1963).

43. See, for instance, W. V. O. Quine, Word and Object (M.I.T. Press, 1960), pp. 222-225.

44. A well-known discussion by Quine on this topic is to be found in his "Two Dogmas of Empiricism", which appears in his From a Logical Point of View (Harvard University Press, 1964).

45. In this connection, it is of interest to note that if we had the sort of ability to discriminate wavelengths of light in the manner in which we discriminate wavelengths of sound, we would be able to perceive an object as being two colours all over at once, just as we can hear two (or more) tones at once, and hear them as distinct. For elaboration of this point, see T. N. Cornsweet, Visual Perception, (New York, Academic Press, 1970), pp. 192-193.

46. Thus I would dispute his indeterminacy of translation thesis, as elaborated in his Word and Object (op. cit.). I will not enter into an extended critique of that thesis here.

47. Wilfrid Sellars, "Some Reflections on Language Games", in his Science, Perception, and Reality (London, Routledge & Kegan Paul, 1963), p. 329.

48. Hence we find that an advanced work on the analysis of counterfactuals (a species of the subjunctive conditionals) is done in the context of intensional or modal logics, making free references to "possible worlds". See, for example, David Lewis, Counterfactuals (Oxford, Basil Blackwell, 1973).

49. W. V. O. Quine, From a Logical Point of View (op. cit.), p. 12.

50. An excellent account of why this is so is presented in T. N. Cornsweet, Visual Perception (op. cit.), chapter VIII.

51. T. N. Cornsweet (op. cit.) provides a clear, reasonable, and more detailed account of these processes.

52. Ervin Laszlo, Introduction to Systems Philosophy (Harper Torchbook, 1973), p. 150.

53. J. J. C. Smart, "Sensations and Brain Processes", in C. V. Borst (ed.), The Mind-Brain Identity Theory (London, Macmillan, 1970), p. 59.

54. D. M. Armstrong, A Materialist Theory of Mind (op. cit.), p. 78.

55. ibid., p. 79.

56. ibid., p. 82.

57. ibid., p. 129.

58. ibid., p. 112.

59. One way in which I might dispute this claim, after I have developed a theory of conscious awareness, would be to point out that it presupposes a subject-object form of conscious awareness, and we may end up having to reject such models of conscious awareness. This point is discussed a bit more when I look at C. A. Hooker's attempted solution to the problem of secondary qualities.

60. D. M. Armstrong, op. cit., p. 208.

61. ibid., p. 209.

62. ibid., p. 216.

63. ibid., p. 217.

64. ibid., p. 217.

65. ibid., p. 222.

66. ibid., p. 223.

67. ibid., p. 223.

68. ibid., p. 226.

69. ibid., p. 226.

70. ibid., p. 256.

71. ibid., p. 256.

72. ibid., pp. 256-257.

73. ibid., p. 257.

74. ibid., pp. 258-259.

75. ibid., p. 260.

76. ibid., p. 272.

77. ibid., p. 275.

78. ibid., pp. 275-276.

79. ibid., p. 114.

80. ibid., p. 282. Also recall that we saw earlier that Armstrong misperceived this problem--see note 19.

81. ibid., p. 283.

82. ibid., p. 283.

83. C. A. Hooker, The Secondary Qualities and Systematic Philosophy (Toronto, Ph.D. dissertation for York University, 1970), pp. 4.27-4.28.

84. ibid., p. 13.11.

85. ibid., p. 4.39.

86. As we saw earlier (p. 167), Armstrong seems to adopt a subject-object (relational) model for consciousness. In his article, "The Nature of Mind"--reprinted in C. V. Borst (op. cit.)--he introduces the theory that consciousness is "nothing but perception or awareness of the state of our own mind" (Borst, p. 78), which again seems to imply the relational model of consciousness. My own present ambivalence with regard to the relational model is in large part due to an awareness of an acute problem for Armstrong's theory of consciousness: Consciousness cannot simply be awareness of a state of our own mind/brain, because there are many aspects of the mind/brain which we are not conscious of; yet the organism is in some sense aware of these inner states, as it uses them in its functioning (e.g. consider the unconscious processes involved in the creative act).

87. C. A. Hooker, op. cit., p. 12.9.

88. ibid., p. 12.10.

89. ibid., p. 15.2.

90. ibid., p. 15.7.

91. ibid., p. 15.9.

92. ibid., pp. 15.9-15.10.

93. ibid., p. 15.10.

94. ibid., pp. 11.9-11.11.

95. ibid., p. 14.1.

96. Bertrand Russell, The Analysis of Matter (New York, Dover, 1954), pp. 263-264.

97. Perhaps the best-known discussion of such objections occurs in J. J. C. Smart, "Sensations and Brain Processes." (op. cit.).

98. This, I take it, is the gist of many of Feyerabend's writings. See, for example, his "Explanation, Reduction, and Empiricism" (op. cit.) and his "Against Method". The latter appears in M. Radner & S. Winokur (eds.), Minnesota Studies in the Philosophy of Science, v. IV (Minneapolis, University of Minnesota Press, 1970).

99. Norman Malcolm, in his Problems of Mind (Harper Torchbook, 1971), pp. 31-59 argues, on Wittgensteinian grounds, that ostensive definitions are not applicable to inner episodes. I will not present my arguments against his claims here, but will instead rest content with indicating that my basis for criticizing views such as Malcolm's is that such views ultimately rest on nominalistic presuppositions, and hence fall prey to my arguments against nominalism (presented in Chapter 2 of this thesis).

100. I owe the point, that ostensive definitions are topic-neutral, to Cliff Hooker.

101. J. A. Schaffer, "Recent Work on the Mind-Body Problem", American Philosophical Quarterly, v. 2, no. 2, 1965, p. 97. At another point (p. 118 of Borst, op. cit.) Schaffer treats the problem as eliminable. Norman Malcolm, however, sees the problem as a serious one--see his Problems of Mind (op, cit.), pp. 65

102. Norman Malcolm thinks that because we cannot establish any location for our mental states introspectively, we cannot establish the contingent identity empirically--see his Problems of Mind (op. cit.) p. 70. But his claim here is based on a misapprehension of the inductive process. That process does not require the direct empirical confirmation of each individual claim, but only confirmation of an entire theoretic system. Indeed, an important aspect of the claims I made in Chapter 5 is that direct empirical confirmation of isolated theoretic claims is not possible in any case!

103. J. J. Sheppard, Human Color Perception (New York, American Elsevier, 1968), p. 98.

104. ibid., p. 158.

105. ibid., p. 99. We may also note that this study of retino topic organization was carried to a fine degree in studies of monkeys by Daniel and Whitteridge. See their "The Representation of the Visual Field on the Cerebral Cortex in Monkeys", Journal of Physiology, (v. 159, 1961)

pp. 203-221. In particular, they found a correspondence between the amount of area of the cortex devoted to a given area of the retina, and the accuity with which an object is seen when it forms an image on that area of the retina. Thus, for instance, the fovea was mapped onto a relatively large area of the cortex, while the peripheral regions of the retina are allocated much less space. Thus the cortical mapping of the retina also serves to explain the percept's variation in detail from the centre of vision to the periphery.

106. This figure is lifted from Arthur J. Gatz, Manters Essentials of Clinical Neuroanatomy and Neurophysiology (Philadelphia, F. A. Davis, 1966), p. 85.

107. Cornsweet, op. cit., pp. 243-244.

108. A much more thorough survey for vision can be found in, e.g., Cornsweet (op. cit.), although there the survey is not conducted with the aim of discovering a specific property to be identified with colour.

109. The account which follows is gleaned from Cornsweet (op. cit.), chapters 8-10.

110. ibid., pp. 246-248.

111. ibid., pp. 247-251.

112. Karl H. Pribram, Languages of the Brain (Englewood Cliffs, N. J., Prentice-Hall, 1971), p. 15.

113. ibid., pp. 18-19.

114. ibid., pp. 66-67.

SELECTED BIBLIOGRAPHY

1. Armstrong, D. M., A Materialist Theory of Mind, London, Routledge & Kegan Paul, 1968.

2. Armstrong, D. M., Perception and the Physical World, London, Routledge & Kegan Paul, 1961.

3. Audi, Michael, The Interpretation of Quantum Mechanics, Chicago, University of Chicago Press, 1973.

4. Berkeley, George, Three Dialogues Between Hylas and Philonous, Bobbs-Merrill, Library of Liberal Arts, 1954.

5. Borst, C. V. (ed.), The Mind-Brain Identity Theory, London, Macmillan, 1970.

6. Broad, C. D., The Mind and Its Place in Nature, London, Routledge & Kegan Paul, 1925.

7. Butchvarov, Panayot, Resemblance and Identity, Bloomington, Indiana, Indiana University Press, 1966.

8. Cornsweet, T. N., Visual Perception, New York, Academic Press, 1970.

9. Daniel & Whitteridge, "The Representation of the Visual Field on the Cerebral Cortex in Monkeys", Journal of Physiology, v. 159, 1961.

10. Duhem, Pierre, The Aim and Structure of Physical Theory, New York, Atheneum, 1962.

11. Einstein, Albert, Relativity, Methuen & Co., 1920.

12. Feigl, H. & Maxwell, G. (eds.), Minnesota Studies in the Philosophy of Science, v. III, Minneapolis, University of Minnesota Press, 1962.

13. Feyerabend, P. K., "Explanation, Reduction, and Empiricism" in Minnesota Studies in the Philosophy of Science, vol. III.

14. Gatz, Arthur J., Manter's Essentials of Clinical Neuroanatomy and Neurophysiology, Philadelphia, F. A. Davis, 1966.

15. Goodman, Nelson, Fact, Fiction, and Forecast, Bobbs-Merrill, 1965.

16. Graves, John C., The Conceptual Foundations of Contemporary Relativity Theory, M.I.T. Press, 1971.

17. Gregory, R. L., Eye and Brain, McGraw-Hill, 1972.

18. Grunbaum, Adolf, Philosophical Problems of Space and Time, New York, Alfred A. Knopf, 1963.

19. Hanson, N. R., "The Logic of Discovery", Journal of Philosophy, v. LV; no. 25.

20. Hempel, Karl, Aspects of Scientific Explanation, New York, The Free Press, 1965.

21. Herstein, I. N., Topics in Algebra, Ginn & Company, 1964.

22. Hooker, C. A., The Secondary Qualities and Systematic Philosophy, Toronto, Ph.D. dissertation, York University, 1970.

23. Hume, David, An Inquiry Concerning Human Understanding, Bobbs-Merrill, Library of Liberal Arts, 1955.

24. Koestler, Arthur, The Act of Creation, Pan Books, 1970.

25. Laszlo, Erwin, Introduction to Systems Philosophy, Harper Torchbook, 1973.

26. Lewis, David, Counterfactuals, Oxford, Basil Blackwell, 1973.

27. Mach, Ernst, The Science of Mechanics, Open Court, 1960.

28. Malcolm, Norman, Problems of Mind, Harper Torchbooks, 1971.

29. Mill, John Stuart, A System of Logic, London, Longman Group, 1970.

30. Popper, Karl, Conjectures and Refutations, Harper Torchbooks, 1965.

31. Popper, Karl, Logic of Scientific Discovery, New York, Harper & Row, 1968.

32. Pribram, Karl H., Languages of the Brain, Englewood Cliffs, N. J., Prentice-Hall, 1971.

33. Putnam, Hilary, "An Examination of Grunbaum's Philosophy of Geometry", in B. Baumm (ed.), The Philosophy of Science: The Delaware Seminar, vol. 2, Interscience Publishers, 1963.

34. Quine, W. V. O., From a Logical Point of View, Cambridge, Mass., Harvard University Press, 1964.

35. Quine, W. V. O., Word and Object, Cambridge, Mass., M.I.T. Press, 1960.

36. Radner, M., and Winokur, S., Minnesota Studies in the Philosophy of Science, vol. IV, Minneapolis, University of Minnesota Press, 1970.

37. Reichenbach, Hans, The Philosophy of Space and Time, New York, Dover, 1958.

38. Russell, Bertrand, The Analysis of Matter, New York, Dover, 1954.

39. Russell, Bertrand, The Problems of Philosophy, Oxford University Press, 1959.

40. Ryle, Gilbert, The Concept of Mind, London, Hutchinson's University Library, 1949.

41. Schaffer, J. A., "Recent Work on the Mind-Body Problem", American Philosophical Quarterly, vol. 2, no. 2, 1965.

42. Scheffler, Israel, The Anatomy of Inquiry, New York, Alfred A. Knopf, 1963.

43. Sellars, Wilfrid, Science, Perception, and Reality, London, Routledge & Kegan Paul, 1963.

44. Sheppard, Joseph J. Jr., Human Color Perception, New York, American Elsevier, 1968.

45. Whitehead, A. N., The Concept of Nature, Cambridge University Press, 1955.